DÆDALUS

The Search for Knowledge

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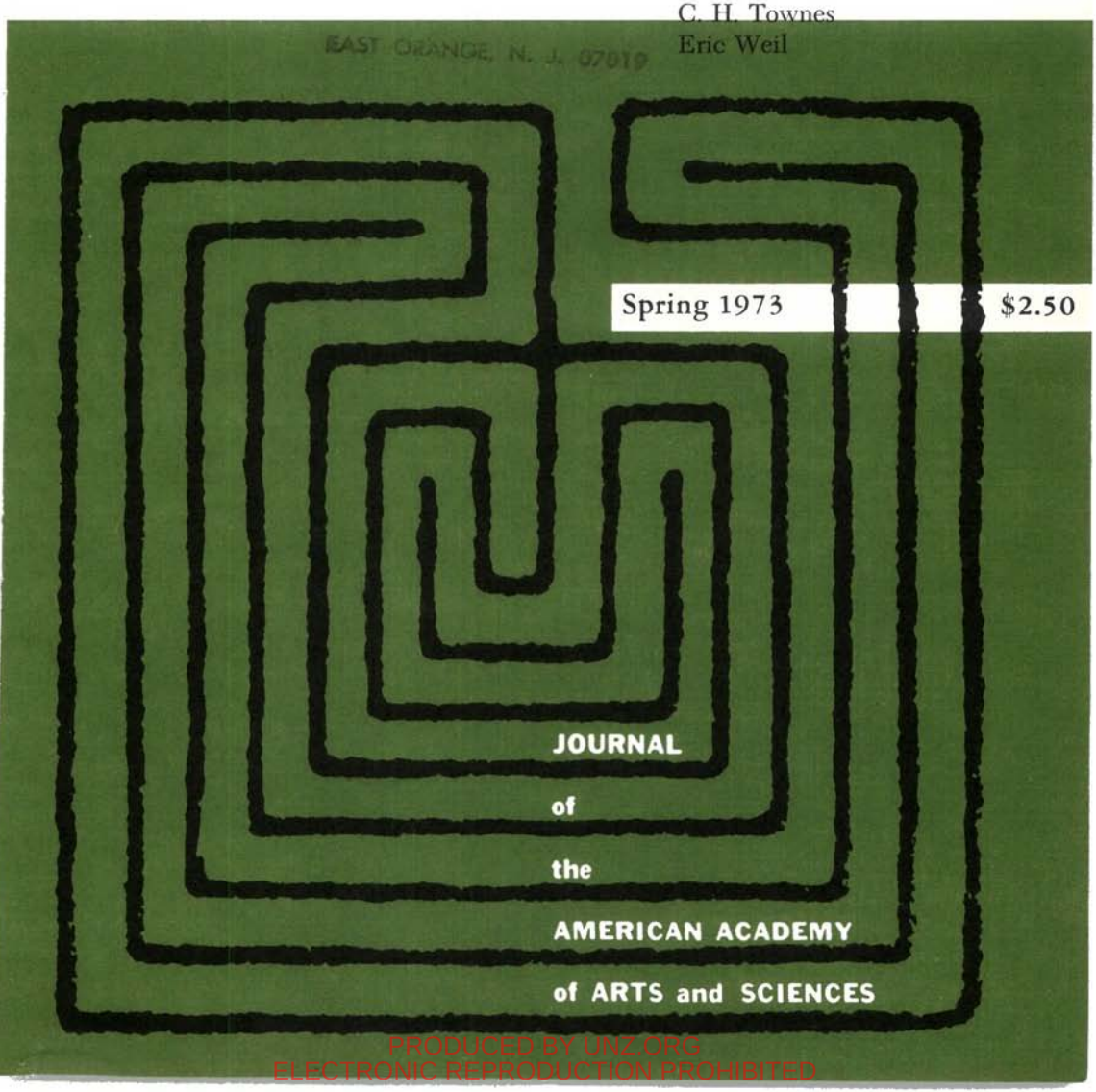

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Spring 1973: The Search for Knowledge
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The Search for Knowledge

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DÆDALUS

Journal of the American Academy of Arts and Sciences

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SPRING 1973: THE SEARCH FOR KNOWLEDGE

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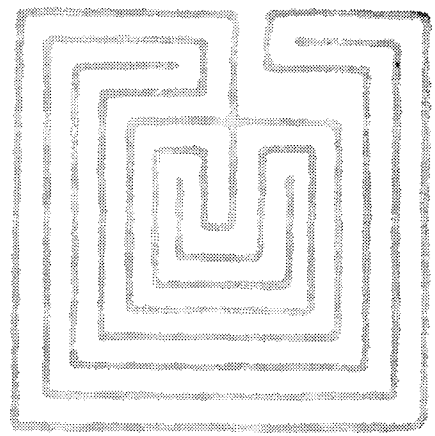

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Preface to the Issue "The Search for Knowledge"

THIS ISSUE of *Dædalus* grows out of an interest that has been long standing in the American Academy of Arts and Sciences. In 1968, when many believed that the universities of the most developed countries, including those of Western Europe, Japan, and the United States, were threatened by forces hostile to traditional concepts of scholarship, a continuing seminar was established in the Academy to deal with the problems of higher education in industrial societies. We thought it important to establish some system of communication among scholars from many academic disciplines whose institutional experiences differed significantly. It was easy, in the midst of the turmoil of 1968, to become lost in university politics, and to be overwhelmed by the agitation that received daily mass media attention. While we never had any intention of avoiding discussion of the specific grievances of that moment, we also felt a need to look at longer-range issues, to consider where scholarship and learning more generally were tending.

In November 1968, a group of scholars and university administrators from France, Italy, Germany, Great Britain, Belgium, Japan, the Soviet Union, and the United States met to consider a whole series of questions relating to higher education. The group's composition militated against the parochial discussion of university affairs, all too common at the time. In the group that gathered at the house of the Academy, it was quite impossible to discuss issues affecting a particular university campus; no one cared to hear about the latest "incident" in some remote corner of the academic world. Inevitably—and happily—the discussion moved to larger themes, of the kind that would have interest for the group as a whole.

Finding a common agenda proved to be not at all difficult. In one way or another, all the participants were interested in how research and scholarship were likely to fare in industrial societies, where access to universities was becoming increasingly open, and where the full implications of the "democratic revolution" in higher education were beginning to be perceived. An attempt was made to determine the kinds of environments that were helpful to scholarly research and the kinds that were inimical to creativity. Talcott Parsons, in speaking about what he called the "research enterprise," insisted that research not be isolated from other activities, that it be seen within a context that included teaching and professional pursuits more generally. In a group where no single academic discipline dominated, there was a need to relate research always to the public happenings that affect public confidence in scholarship and that influence public funding. Especially given the academic atmosphere of 1968, it was impossible to avoid discussion of university disorders, but the meeting was not dominated by those who imagined that their institutions were being sapped and that all attention should be directed to specific dangers.

By the time the second meeting took place in Paris in June 1969, few evidences remained of the turmoil that had overwhelmed France the preceding spring; the

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street demonstrations, strikes, and building occupations seemed very remote, part of another era. A year earlier, everyone had agreed that the French universities would never be the same again, that wholly new methods of organization, instruction and governance would prevail. There had indeed been some reform—on the statute books it appeared substantial—but the seminar sensed how much in fact remained the same. Since the dramatic confrontation in France had produced such limited results, it became easier to believe that the kinds of issues that the group was inquiring into were not at all abstract; they would be discussed long after student unrest and university violence had receded from public attention. Asa Briggs expressed a view shared by others when he said that he did not expect governmental officials in Whitehall or Paris to come up with a plan for the whole of higher education. Individual institutions would have to remain "centers of initiative"; they would operate effectively only if individuals accepted the necessity of diversity.

This issue of *Dædalus,* entitled, appropriately enough, "The Search for Knowledge," grows out of the discussions that began during those agitated years and have continued down to the present. Uniting in a single volume areas that are too often separated—the humanities, the social sciences, and the natural sciences, biological and physical—gives us an opportunity to suggest not only the intrinsic differences among them but also the similarities which make them part of a single intellectual enterprise. It is difficult to know whether the differences are more significant than the similarities. There has been no effort made to represent all the academic disciplines; such a venture would require a volume many times the size of this one, and would serve little purpose. Neither was the object to describe the state of research in a specific discipline. Rather, it was hoped that a great variety of fields would be studied, and that an effort would be made to suggest how public policy, private philanthropy, individual scholarly initiative, popular taste, institutional supports, tradition, and habit all influence research. Certain of the articles have an almost autobiographical quality. This was, of course, intended. It will not escape the reader that many of the distinguished scholars here represented are iconoclastic and daring in their recommendations for innovations in research. These are not the papers of self-satisfied scholars, anxious to celebrate what they or their colleagues are accomplishing. Indeed they make many implicit and explicit criticisms of prevailing norms.

The tone for the whole issue is set by E. H. Gombrich, who believes that the humanist's task is to recover, preserve, and interpret the cultural heritage of mankind. He sees the humanities as diverted from their proper course and undermined by timidity and anxiety as the result of the worship of false idols, which give too much importance to the quantitative, the superficially novel, the fashionable, and the academically respectable. His opposition to disciplinary "territorialism" is admirably stated when he says: "An alert and industrious scholar can acquire the skills to investigate nearly any question which arouses his curiosity." Such would be possible, however, only in communities where the young and the old mixed, and where they were in a position to exchange ideas. Gombrich does not deny that his prescription for institutional reform

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comes very close to asking for replications of the Warburg Institute with which he has himself for so long been associated.

Northrop Frye is more openly autobiographical. He describes how the Ph.D. model in the humanities has mistakenly patterned itself after the doctorate in the natural sciences. When the specialist is relieved of all obligations to teach undergraduates in order to teach his special field to graduates, and when pure research is considered the highest possible objective, then the university, according to Frye, tends to become a "social counter-environment." He describes the study of literature at Oxford in the 1930's, where only philological inquiry was prized, and the reaction in the 1940's with the arrival of the so-called "new criticism." Now, he believes, the exaggerations of that movement are safely over, and a new kind of specialization is emerging, different from the old pseudo-scientific kind. Aware that the printed word itself is thought to be obsolete today by those who exaggerate the educational potential of the electronic media, Frye defends the book as indispensible. He speaks of the electronic media as producing a "superficial alertness combined with increased difficulty in preserving the intellectual continuity that is the chief characteristic of education." The purpose of the study of literature, in Frye's mind, is to lead the student from his "present subjective social vision into the total social vision of mankind."

Eric Weil makes an even more explicit defense of the humanities, particularly of history and philosophy. Against those who follow Thorstein Veblen in believing that the humanities are "anachronistic," he sets his aim. The humanities, in Weil's mind, are the essential part of an individual's education; only they provide him with an understanding of himself. For Weil, the humanities are not an adornment to be added on as an afterthought when the individual has learned to do what is really important—to earn his living. That, in his view, is totally to misconstrue the nature of modern society. Weil dwells on the increasing importance of leisure today, and on the failure of institutions to teach men and women to live with their freedom. He would not have universities abandon their "indispensable work of instructing and of intellectually disciplining the young," but he does not see this as their central or exclusive task; Weil writes: "[universities] should try to become places where grown adult men and women can discuss among themselves and with adult specialists what occupies their minds, and occupies it at present. . . ."

Joseph Ben-David's recommendations for reform in the social sciences are no less drastic. In his view, too many of the social sciences borrow models from the natural sciences (from physics, in particular) and then ineptly use them to produce theoretical constructs. It is ironic, he says, that the social sciences, which deal with phenomena of the greatest complexity, borrow from sciences that deal with phenomena of the least complexity. Unimpressed with much of the basic research in the social sciences which he finds "vague and variable," he thinks that a remedy lies at hand: social scientists need to adopt more eclectic approaches like those characteristic of clinical medicine and engineering. Interdisciplinary teams, he says, are of limited worth; what is required is a total overhaul of social science curricula. With it must come substantial improvements

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in our systems of information storage and retrieval. He concludes: "Our only means of avoiding parochialism and our only hope of building substantively important and interesting theories is to encourage the growth of a thriving comparative research." Social scientists, he explains, should be more concerned with how their ideas are put into practice, not because it is important that they themselves be socially active, but because their methods and models would gain precision and integrity through being tested in practice.

Ronald Dworkin, in looking at research in jurisprudence, a discipline that lies between law and philosophy, dwells on differences between academic law in the United Kingdom and the United States. He contrasts two styles which he characterizes as "limited" and "broad." The "limited" style, neither easier nor more mechanical than the other, depends essentially on materials available in a law library; it is particularly suited to dealing with questions of comparative law and legal history. The "broad" style presupposes a familiarity with certain social scientific disciplines outside the law. This approach has been gaining favor in the United States. Using the school busing issue as an example, Dworkin suggests why the "broad" approach, which calls for more than a purely legal competence, and which admits the utility of knowledge gained from philosophy, psychology, economics and sociology, is particularly compelling. To date, Dworkin, explains, a great effort has been made to instruct lawyers in the social sciences. He asks whether more might not be gained by reversing the process, *teaching law to the philosopher and economist rather than philosophy and economics to the lawyer*. Such a proposal would require curricula changes of a kind that Dworkin describes.

Harry Johnson, in considering the "research revolution" that has taken place in economics since the end of World War II, is particularly anxious to explain why the breakthrough of the early 1950's occurred among "English-language economists," and why it was centered in the United States. In comparing the situation in the United States, the United Kingdom, and Canada, Johnson describes how national cultures and the economists' relation to them affect research, as well as how tenure and promotion systems and methods of funding determine the character and quality of research. There is no reason to believe, he says, that present methodological tendencies will continue; he writes: "It may well turn out, in the near future, that the possibilities of acquiring useful new knowledge by further research along scientifically respectable computer-and-mathematical-oriented lines will play out, and that the world will discover that what it needs, after all, are political economists and not economic scientists."

Because several of those who have written about the humanities and the social sciences have mentioned how the methods of the natural sciences, introduced into their own disciplines, have tended to distort research, it is gratifying to have S. E. Luria's essay suggest that influence can flow the other way as well. It is obvious from what he has written that he himself has been greatly influenced by what he has seen of the research habits and styles of both humanists and social scientists. This is not to say that he has actually borrowed the methods

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of either; rather, his political and societal concerns reflect interests that are common in a university environment where it is impossible for a single disciplinary interest to predominate. It is significant that Luria thinks the university almost unique; he sees it as "the optimal setting for research," and suggests that it is particularly suited to research on controversial subjects. In his view, the public's understanding of science (and of research, more generally) is woefully inadequate. The mass media cannot be expected to remedy this situation, he says; rather, the universities ought to do so.

John Eccles writes about the situation in the biological sciences, paying particular heed to the neurosciences. Just as the general public often confuses science with technology, so also its view of science tends too often to be based on what it knows (or reads) about physics. Eccles is very anxious to distinguish between the situations in biology and in certain branches of physics; he particularly emphasizes the character of biological research teams. As he explains, in his experience, the optimal size for a scientific team in biology is two to four researchers. Although it differs from "big science" in this respect, it resembles all science in another regard; Eccles writes: "I feel that one cannot stress too strongly the importance of the elite in the various branches of science. One can observe the rise and fall of different branches of the neurosciences in a country according to the kind of leadership that is provided." If Eccles believes in the necessity of scientific elites, however, this is not to say that he thinks they are always well used. Freedom, he writes, is of paramount importance for the scientist, especially the freedom to travel; provincialism is the hazard to be constantly guarded against.

David Robinson, responding to the notion that university science is dying, calls the report greatly exaggerated. Paying particular attention to the situation in high energy physics, Robinson reviews circumstances in the 1950's which led to greater governmental involvement, but which, nevertheless, left control of the vast new accelerators in the hands of university-based scientists. Even the central laboratories, which are national facilities, are, according to Robinson, largely organized to support university research groups. There is little evidence to suggest that this is about to change; as Robinson explains, the university scientists are still "light on their feet."

J. B. Adams, Director-General of CERN, describes how the whole style of research in nuclear particle physics depends on expensive and complicated apparatus. The field did not decide to become "big"; its bigness was imposed by the nature of the investigation it was making. Reviewing the history of the development of nuclear physics from its simple beginnings early in the century, Adams explains how the Second World War gave the discipline its special aura and led to its involvement with central governments. The scientist working at CERN, he says, is working under conditions very different from those that obtained at the Cavendish Laboratory in Cambridge even as recently as the late 1930's. The massive technology and complex organization must affect the kinds of research that take place. Given the enormous expenditures involved,

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it is impossible for such research not to be subject to external pressures. An entirely new kind of scientific enterprise results, with quite different restraints and very different satisfactions.

Scientific research demands a commitment of resources vastly different from any that occurs in either the social sciences or the humanities. Inevitably, there is continuing concern with achieving the maximum return for a given amount of human and material investment. This concern, Harvey Brooks explains, preoccupied all the major industrial societies during the 1950's and 1960's, a time when the problem was essentially one of production. More recently, however, a more difficult problem has presented itself. How can scientific knowledge and research be related to desired policy ends? What can be done to help scientific institutions couple their knowledge to socially useful action? Growing dissatisfaction with the performance of scientific institutions in the social utilization of knowledge has led to various kinds of organizational innovations. Brooks writes about several of these, and explores particularly the question of whether government funds for scientific research are best distributed by a single autonomous science agency or by numerous agencies, each making its own decisions about what research to sponsor. He shows, also, how this debate over the organization of science and technology at the national level is reflected in analogous debates occurring within large industrial corporations. "The central problem of scientific organization," he writes, "is how to reconcile the need for autonomy and integrity in its own internal process of exploration and self-criticism with the demands of society that the fruits of science be guided into channels which society deems beneficial."

Lewis Branscomb has many of the same concerns; as he explains, ". . . our most serious failure is not the inadequacy of our commitment to learning the truth; it is our failure to bring about action on the basis of that understanding to achieve the conditions for a reasonable future for the earth's inhabitants." He is persuaded that self-motivated university research alone will never achieve such purposes. There must be public funding, he says, for many different kinds of research institutions, both public and private. New kinds of institutional partnerships are required to join bodies that have never previously cooperated. Branscomb mentions, as an example, the possibility of links between governmental laboratories and universities. There is an imperative need for a sharing of information on both national and international levels of a kind that has not heretofore existed.

C. H. Townes, looking at the way science has developed in the United States, contrasts the research that goes on in the university with what takes place in government laboratories, industrial laboratories, and privately supported research institutions. These laboratories are created for different purposes; they recruit scientists with different kinds of interests. Thus, for example, Townes suggests that only private research institutions can have a commitment to basic research comparable to that of the university. Industrial and government laboratories, by comparison, are particularly suited to applied research, and if the others try to enter this area they do so at a great disadvantage. Laboratories,

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depending on their type, offer different career options; their personnel policies are necessarily distinctive. Townes recognizes the possibilities of collaboration between different kinds of laboratories, but he does not underestimate the difficulties.

Peter Kapitza, in writing about the organization of science in the U.S.S.R., provides immensely valuable information on what he thinks is the most crucial problem: the selection and training of scientific personnel. For him, the future of science depends on the recruiting of exceptionally talented young people and on their participation in a very rigorous training program, which he describes in some detail. He is persuaded that scientists alone are able to determine the direction that scientific inquiry ought to take, and that it is incumbent on them to make their decisions in accord with national and international needs.

We owe a great debt to the Ford Foundation for making this issue of *Dædalus* possible. Without the generous support granted the American Academy of Arts and Sciences by the Foundation to support not only the issue itself, but the seminars and discussions that led up to it, there would have been no possibility of having humanists, social scientists, and natural scientists join together in an enterprise like this one. Thanks are due also to Mme. Adriana Salem who opened her home in Paris to the Academy on many occasions for meetings that brought together European, American, and Asian scholars.

S. R. G.

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E. H. GOMBRICH

Research in the Humanities: Ideals and Idols

THE TERM "RESEARCH" is used indiscriminately to describe activities in the sciences and in the humanities. I can only speak with competence about the latter, but I believe that the role of research in science differs fundamentally from that with which I am more intimately concerned. The idea that science represents what used to be called "a body of knowledge" has become obsolete. It is not the facts stored up in textbooks that constitute science, but the questions that are asked and the problems that demand solution.[1] Seen in this light, science is an activity, and research is of its essence. It can only continue to exist if it continues to advance. The scientist is like the rider on a bicycle; he could not remain in his seat if he had to stop.

Certainly the humanities should not remain stagnant either; and though I shall argue in this paper that the assumption that they should emulate the sciences in matters of research can lead to grave distortions, I should like to reassure my colleagues that I am not going to let down the side and argue for the curtailment of the small funds we have been getting. Still, the notion of a body of knowledge is less inapplicable to some of the humanities than it is to science. The humanists of the Renaissance were concerned with a group of texts and monuments which they wanted to recover, preserve, and correctly interpret. It was their aim to master the languages of these texts and to expand their references because they admired the civilization of the ancient world and wished to profit from its heritage. The range of texts and monuments which concern the humanist today has enormously widened, but his basic motivation is the same. His concern is still with languages—taking the term in its wider sense—and any vocabulary constitutes a "body of knowledge." The editor of this issue, presumably, still expects its readers to connect something with the name of *Dædalus* and to appreciate its association with the schematic labyrinth on the cover. It is this type of knowledge that is kept alive by the humanities without the benefit of research.

Take my own field, the history of art; without some knowledge of the Bible and of the principal stories from the classics, the work of Michelangelo or of Titian would become mute. If you do not connect the name of Moses with anybody in particular, Michelangelo's statue in S. *Pietro in Vincoli* would look odd indeed, and if you had never heard of the Loves of Zeus, Titian's *Europa on*

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the Bull would be even more puzzling. This is not a matter of doing research in iconography, simply of knowing the symbols current in a civilization. Admittedly it is all a matter of degree. Nobody can expand all these references, and part of scholarship consists in the knowledge of how to use dictionaries—using the term in a slightly extended sense.

As long as a civilization feels sure of its values, the need to make contact with these canonic texts and monuments of the past need not be argued. I am old enough to remember the generation of doctors and lawyers, civil servants and bankers who read the classics as a matter of course in the original language, and who could be described as genuine humanists though the idea of research may never have entered their minds. Even teachers at school or dons in the old universities saw it as their principal mission to pass on their knowledge, their enthusiasms, and indeed their veneration for this heritage without feeling called upon to make fresh discoveries.

Admittedly this unquestioning acceptance had its dangers. The guardian of canonic texts can easily become the pedant whose concern with irrelevant trifles has been ridiculed for so many centuries. It has always been the justified charge against the pedant that he killed the texts he was meant to keep alive. Where lively minds are engaged with the heritage of the past, humanism will also remain lively. Even those educated laymen to whom I referred never regarded the texts of the classics on their shelves as dead matter. They were part of their intellectual universe, the objects of love or, maybe, of aversion; there were reputations to be questioned and interpretations to be challenged.[2] These people, also, were the natural public of the professional scholar whose editions they used and whose views they debated. What distinguished the professional academic was mainly that he had more leisure and more technical competence to engage in such constant revisions. It was the mark of the real scholar that he could not read a text or look at an image without being aware of the depth of his ignorance and of the need to find out more. Sometimes such a search would take him to a library, occasionally also to an archive; he might decide to wade through certain records for years in search of an answer, or just to store the problem away in his mind hoping that the solution would turn up one day in an unexpected place. It was in this way, on the whole, that the scholars of the past conducted what we now call "research."

Indeed one would hardly use the term when describing the life style and activities of an archetypal scholar and humanist such as Jacob Burckhardt. His lectures and his books testify to the immense range of his systematic reading, and nothing he touched could remain inert. But his immense productivity lay in his capacity to illuminate and reinterpret the texts and sources rather than in the discovery of new ones. I believe we must distinguish such inspired productivity from the idea of "output" or "results" if we are to understand the peculiar problems of the humanities.

The contributors to this issue were asked, "In what fields does the nineteenth century model of the scholar working by himself still predominate? What

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confidence is shown in this model through the ways monies are appropriated and recognition awarded?"

I have expressed a certain nostalgia for the model of the nineteenth century scholar, but I do not think that any ways of appropriating monies or of awarding recognition would be likely, by themselves, to bring back what was essentially a style of life. Take the life-work of Burckhardt's successor, Heinrich Wölfflin, the great interpreter of stylistic developments in art. It is hard indeed to imagine him receiving a research grant to study the compositional devices of Raphael or being influenced in his search by any thought of recognition. True, as the occupant of the Chair in the University of Basle he was free not only from financial worries, but very likely also from the many pressures which nowadays beset a department head.

When I was a student in Vienna old Wölfflin paid a visit to my teacher, the great scholar Julius von Schlosser. As they both emerged from the Professor's *sanctum* where they had been closeted, the host pointed to the rooms of his Institute where we were sitting and working and said with more candor than tact: *Also hier beginnt das Grauen* ("this is where the horror starts"). The horror, that was us—though these professors certainly knew how to keep their students at arm's length.

Much has been written about the authoritarianism of the old-fashioned German University Professor, and the abuses of the old system are too obvious to need laboring. What is relevant in our context is only that for all its faults the tradition was good for scholarship.[3] There are two diametrically opposed conceptions of a university which somehow got mixed up in our time. The first may be said (schematically) to be derived from the continental medieval system in which the university provided a "chair," a pulpit even, for the eminent man who can be persuaded to pass on his knowledge to those who are willing to "sit at his feet." If he disappoints them, they can go elsewhere (as I left Schlosser's lectures for a term to hear Wölfflin's in Berlin). There is nothing wrong, in the abstract, with this idea that a great man has the obligation to take some time off from his work for the benefit of his disciples. Alas, there can never have been enough great men to fill all the chairs and so we got the small men who had to assume the pose of eminence and were corrupted in the process.

In the English college system this danger was less acute; for the don was primarily a tutor to the young gentlemen who were to become lawyers and clergymen. While the loyalty of the continental professor was to his subject, that of the English university teacher was really to his students who stayed under his care unless they were "sent down." Morally and socially there is much to be said for this conception of teaching, but the attempt to marry the two systems may be responsible for the perplexities which are the topic of this issue.

Our system of appointments and promotions in the Anglo-Saxon world is still based on what might be called the assumption of eminence. Indeed the letter of appointment used by my university quite properly includes among the duties of the recipient that he or she should "advance the subject." Yet we have also

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taken over from the college tutor the burdens and responsibilities of caring for the intellectual and spiritual welfare of our students. It is well-known that the tensions and conflicts between these two responsibilities have resulted in much restlessness and unhappiness. It was bad enough for the German Professor to feel that he was expected to be a great man. It is much worse for the ordinary member of a university staff to know that his worth will be measured by the number of papers he has published and the symposia he is invited to attend. It is these pressures that result in what is often called the academic industry, an industry that only rarely "advances the subject" and quite often impedes its growth.

It is easy to decry this industry, but less easy to remedy its shortcomings. After all, most of us are part of it. We live in a world that values results. The Annual Reports of our Departments and Institutes should present them as veritable beehives of activity; more offprints are sent round and received in return than any of us will ever have a chance of reading; we are on the boards of learned journals and are even guilty of writing books. Like most industries, ours finds itself confronted with the problems of overproduction and—if the truth is to be confessed—with the danger of polluting the environment. The "fall out" of the academic industry is the pretentious jargon that seeps into language. More depressingly even, its overheated apparatus appears unable to adjust its output to the needs it was once designed to meet. There are too many areas where we fail to live up to our responsibility as humanists which I defined as that of recovering, preserving and interpreting the cultural heritage of mankind. Today no less than in the past these obligations can only be met by those who have mastered the requisite body of knowledge, and we all know that even the knowledge of classical languages is declining at an alarming rate. An even more glaring instance of such a breakdown is the fate of the monuments and manuscripts of India. Many of them are disappearing through pillage and neglect before their existence has even been recorded.[4]

It is important to realize that scarcity of funds is only one element in situations of this kind. Money is neutral. It can aggravate the ills of overproduction as easily—or more easily—than it can be used to cure these serious malfunctions. What is needed in the humanities is not yet another lobby for more grants and research projects, but rather a forum for the exchange of views on what constitutes worthwhile research, and what dangers threaten to distort its progress.

Francis Bacon in his *Novum Organum* listed four groups of idols the worship of which he found detrimental to the progress of science. I know that his vision of science has been criticized as naive, but he was a great propagandist who knew how to put his ideas across.[5] I shall take a leaf out of his book and list four classes of idols which, in my opinion, divert the humanities from their course.

I propose to identify as my first group the *idola quantitatis*. These are in fact set up by the very conception of science with which the name of Francis Bacon is connected, the creed of "inductivism" which claims that truth emerges in

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the form of generalizations based on the accumulation of data. This, as we know, is an idle dream. There are no neutral data; we can only collect evidence if we want to bring it to bear on a particular hypothesis, and in doing so we must never forget to ask whether the game is worth the candle.[6] Evidence for the proposition that "all men are mortal" need not be sought by putting all birth and death registers preserved anywhere in the world through a supercomputer.

Of course both in science and in the humanities there are collections of data which save a lot of trouble. I have referred to dictionaries as a case in point. The importance of other tools of research such as reference books, bibliographies or editions of texts need not be labored. But those who gratefully turn to these works also know that it would be quite pointless always to demand completeness. It is the insistence on this demand, the belief that the recording of all available data must precede all other research in the humanities that I associate with the cult of the *idola quantitatis*. Do we need a corpus of *all* doorknockers? I do not raise this question because I find doorknockers particularly uninteresting. On the contrary I have been interested in the frequency with which they show threatening masks, snakes or other apotropaic devices. The question is only whether we would gain a lot by a statistical survey of all doorknockers made, say, before 1700, to establish the exact distribution and relative frequency of these motives. Even so, a project of this kind might easily find institutional support; teams could be organized and photographic archives established, though by the time the project approached completion the problem of magic survivals might interest nobody and the "use" to which this campaign was to have been put might be entirely forgotten. What scholars might want to know by then, might be the relation of doorknockers to the coming of the electric doorbell, and for this question the whole enterprise would have proven useless. "It has to be done all over again"—or has it?

The worship of the *idola quantitatis* is not only sterile, it induces sterility in its devotees, especially the young. How many good ideas must remain still-born because the priests of the cult demand that they should be established by induction. It is this inductivism which produces the feeling among the young that they will never "know enough," and which will ultimately prevent them from daring to ask any worthwhile question. It diverts them to the tiny plots of specialists who know "more and more about less and less."

My second group of idols may be known as the *idola novitatis*. Here, too, the emulation of the sciences leads to distortions of true productivity. If, as I have argued, the humanist remains to some extent a guardian of texts, novelty is not his prime concern. True, there is something exhilarating in discovering a fresh interpretation of a term, or a turn of phrase in a well-known text which has escaped countless commentators and which illuminates a forgotten meaning. But even the best of these conjectures or discoveries must take its modest place among the footnotes. The commentary remains subservient to the text. In this respect the humanist as an interpreter of our heritage has something in common with the performer who interprets a text on the stage or in

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the concert hall. We want him to be the faithful mediator rather than the purveyor of novelty for its own sake. It is easy to propose a fresh reading if we treat a text cavalierly and disregard the intentions of the creator, if, in other words, we prefer the new to the true. Unfortunately, such is the fabric of the academic industry that even an absurd reading will be accorded immortality in the footnotes. Anyone who can get a reputable journal to publish a learned paper purporting to prove that the Mona Lisa represents Cesare Borgia in disguise would at least be discussed in all subsequent treatments of the theme. Those who doubt it should remember that Marcel Duchamp's schoolboy prank of painting a moustache on a reproduction of that portrait still figures in survey courses—and in this paper.

Close by the *idola novitatis* in the temple of learning I find the *idola temporis,* the idols of the age. By this term I refer to the lure of newly developed intellectual and mechanical tools which seem to promise prestige to those who "apply" them to the humanities. I am anxious not to be misunderstood here. It is not the tools and techniques I wish to question under this heading. I am no more concerned with the validity of such movements as Marxism, psychoanalysis, or structuralism than I am out to criticize the use of film cameras, tape recorders or computers in those fields where they yield relevant data. It may certainly be argued that the coming of these tools has placed the traditional humanities at a disadvantage. Who would not want to have a tape recorded interview with Shakespeare or a film of the first performance of Hamlet? It is altogether characteristic of the humanities that our evidence is very fragmentary and that we have to make do with accidental scraps of information. We must resign ourselves to the fact that we know nothing about Shakespeare's sex life, eating habits, earnings, and politics, and that we therefore cannot put his life and work through the paces of any of the current theories of human behavior. Sociologists, psychologists, anthropologists and linguists will naturally select a type of material where the evidence flows more abundantly. It is more rewarding, in many respects, to investigate monasticism in modern Ceylon by interviewing a large cross section of monks about their beliefs[7] than to study the Cistercian monasteries of medieval Europe. But, though the records and monuments of this movement are far too incomplete to offer suitable material for any of the modern approaches, the humanist will not want to neglect them. After all these monks helped to shape the physical and intellectual landscape we now inhabit. We study them in our constant efforts to assess our own heritage.

Of course we try to learn from the results of any science, but we can only do so if we know our place, our strength, and our limitations. There is nothing more dreary than the mechanical application of any formula to one topic after another. Vulnerability to intellectual fashions is the most depressing effect of the pressures created by the academic industry. Here, too, the cult can be particularly harmful to the young who are so easily impressed by a fresh and prestigious jargon. If the *idola novitatis* may at least encourage a somewhat

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hectic originality, the *idola temporis* merely promise originality to enmesh their worshippers in a predictable conformism.

If these warnings sound conservative, I hope to dispel this impression by identifying the last cluster of idols, the *idola academica*. They are the idols that have established themselves in academic life in the very process of teaching the humanities. For administrative reasons universities tend to be organized in departments which offer a syllabus leading to examinations. Alternatives have been tried, but I do not think there is much to be said against this system as long as it is applied to the acquisition of skills. Whether we want to learn to drive a car, play the flute, speak Chinese or read cuneiform, we must be guided through the various phases of proficiency which can be tested reasonably well. Even such traditional faculties as theology, medicine and jurisprudence require certain skills as qualifications and may well be suitably organized for their teaching and testing.

The so-called "arts subjects" however, are certainly distorted through this division into "disciplines" and "departments." It is true that I have argued that the humanities represent to some extent a "body of knowledge," but bodies do not survive dissection. Shakespeare's sonnets are studied in the English department, Petrarch's in the Italian, the sonnet form in that of Comparative Literature, and Petrarch's Latin works in no department whatever. What is at issue is not the need for "interdisciplinary" studies. I have argued elsewhere that this very monster of a term begs the question because it implies that there are "disciplines."[8] The alert reader of any text must inevitably ask questions which will take him into linguistics today, into history tomorrow and probably into social studies next week. Obvious though this is, a student of any syllabus would probably still be discouraged from doing "research" on any topic that falls outside of it, and if he persisted he would encounter what Aby Warburg called the frontier guards who would ask for his credentials. There is more of academic trade unionism in these demands than is often acknowledged. An alert and industrious scholar can acquire the skills to investigate nearly any question which arouses his curiosity. Thus, Aby Warburg learned enough of astrology and Rudolf Wittkower enough of the theory of music and proportion to transform, respectively, iconology and the history of architecture.

I fear that the insidious effects of the *idola academica* extend beyond this timid territorialism. Teaching a syllabus inevitably creates a bias that carries over into research. The course becomes the thing. It must be supplied with material, the machine must be fuelled, and topics will be scrutinized with this unconscious bias. The authors who figure in the syllabus must serve as pegs onto which to hang suitable classroom themes. The sabbatical leave of six months may just be long enough to carry out this little piece of research that will be useful also to colleagues teaching the same prescribed text. Where this is the attitude of the teacher, his students are likely to gain a similar conception of what is expected of the research worker. His efforts and his methods in turn should ultimately be geared to the teaching of the subject. The subject of his

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Ph.D. research on which he will agree with his teacher will naturally be marked by this bias.

The importance attached to this exercise which has notoriously increased during the last few decades has come in for much justified criticism.[9] I do not think, however, that the institution is necessarily harmful, as long as it is regarded as genuine apprentice work. Those whose job it will be one day to supervise research can be expected to prove that they have mastered the basic techniques. But instead of being a kind of "driving test," the Ph.D. thesis has come to resemble one of those terrible ordeals so frequently connected with rites of initiation. The priesthood of teachers sometimes seems bent on guarding the prestige of their order by making entry difficult and painful. Thus the formative years of a young scholar's life are frequently taken up by an effort which serves, in the official phrase, "to satisfy the examiners." By the time he has done so, the clay has hardened; he is committed to a line of research which he may find difficult to abandon. Hence that tendency to self-perpetuation and that overcrowding of certain fields which we have observed to be matched by the neglect of others. The humanistic subjects which must be taught in every culture for good educational reasons—Dante in Italy, Goethe in Germany, Shakespeare in English-speaking countries—are squeezed dry for research topics by those who seek academic employment. A type of periodical has sprung up listing "research opportunities" in these overgrazed areas, while a few steps beyond their confines virgin land is crying out to be cultivated.

Nor is this unbalance the only negative effect of the *idola academica*. The very techniques of the classroom, the way students are inducted into the reading of an author or the appreciation of an artist, has come to influence the style and the aims of research. The extent to which the routines of teaching sometimes guide and distort the practice of research seems to me particularly characteristic of the humanities. The refusal of the German Professor to adapt his lectures to the capacity of his students may have been morally reprehensible, but the admirable effort of the devoted teacher to adjust his approach and his thinking to the needs of newcomers may be more disastrous in the end. Pedagogic devices developed to engage the interest of beginners become elevated into methods of research. I am here thinking of the analysis of formal relationships in painting, of images in poetry, or of the meaning of words in philosophy. They may be indispensible for the teacher who wishes to fill an hour and to compel his class to attend to a text or to a picture, but they are unlikely to "advance the subject."

How, then, can this desirable aim be furthered? The wind bloweth where it listeth, and I do not think that the growth of good ideas can be organized; but it is not impossible that we can make a conscious effort to create a climate in which their growth will not be stunted. Much that is sterile in the academic industry of today springs from simple anxiety. The existence of academic primadonnas or super-stars, the publish-or-perish complex, the impossibility of "keeping up" with the literature, the crowded schedules, and the increasing demands of a home life based on "do-it-yourself," make one wonder how those admirable

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young scholars one still encounters managed to come through at all. But too many seek shelter in "safe" topics, while others hide their insecurity behind the façade of aggressive novelty hunters. Those who want to cure these and related conditions must tackle them at the root and remove the anxiety. In this respect the creation of havens for eminent scholars is no remedy. It is a strain even on the elect to be cast into the role of the sage who merits a perpetual sabbath.

What is needed are mixed communities of scholars where younger and older people can freely mix and exchange ideas without being either over-impressed or under-impressed. Communities, the members of which are not so totally exempted from ordinary chores and routines that they must ask themselves day and night whether they have lived up to their position, but where they are not ground down by the demands of teaching and administration either. In such a community there are no "stars" who are regarded with false awe, nor need a beginner be afraid to air his views. Everyone should feel free to follow his hunches in any direction without the fear of being told that this is not his business. Only in this way can it happen that once in a while a fresh perspective is opened which suggests a new line of research in the humanities.

If this prescription sounds uncommonly like an idealized portrait of the Warburg Institute, this may be due to the fact that I am only familiar with my own parish. What I think I have learned in my 36 years of association with that group of scholars is that such an institution is comparatively inexpensive. I do not want to sound unappreciative of the University of London which has provided and continues to provide the running cost of an institute the purpose of which is not easily explained in terms familiar to financial committees. I would not dream of suggesting either that an institute of this kind could not do with a larger annual budget. What I want to stress for the purpose of this issue is merely that in comparison with the cost of cyclotrons and computers, even a first class library in the humanities needs no exorbitant grants, particularly if the purchasing remains firmly in the hands of a very competent scholar-librarian who knows how to be selective. Admittedly, here my experience is perhaps a little misleading. The Warburg Institute Library is based on the stock of books collected by Aby Warburg and maintained for several years after his death by the Warburg family. Moreover, we happen to benefit from the proximity of the British Museum Library with which we need not and cannot compete. But users of our library have found that it not only serves research, but also generates fresh ideas, as does our photographic collection. To follow up these ideas the handful of scholars who form the institute's academic staff certainly need time, but they are quite happy to teach both undergraduates and postgraduates as long as the pressure is bearable. Add a modest sum for traveling for which they can apply, and the provision of junior and senior research fellowships without which the continuity of the institute's work would be in danger, and the financial requirements of the institution will be satisfied.

I believe that in terms of cost benefit the contribution of such a place to the advancement of learning need not compare unfavorably to that of larger cen-

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ters. It is true that any intellectual non-conformism must involve the adventurer in the risk of academic isolation, but there is no guarantee today that it is the overcrowded fields which offer the best chances of employment. I hope indeed that even on this practical score we need have no qualms in encouraging the young to exchange the cult of the idols for the true religion of scholarship.

REFERENCES

1. For this view of science and its indebtedness to K. R. Popper see the paper by John Eccles in this issue.

2. See my essay "The Tradition of General Knowledge" in Mario Bunge, ed., *The Critical Approach to Science and Philosophy* (New York and London: The Free Press of Glencoe, 1964), pp. 431-444.

3. There is a vivid description of the good and bad sides of the system in Richard B. Goldschmidt, *The Golden Age of Zoology* (Seattle: The University of Washington Press, 1956), chapter I.

4. There is little consolation in the fact that the authorities on the spot are partly responsible. The small band of scholars who try to stem this tide find little support either East or West. The American Academy in Benares which was working on a comprehensive photographic record of monuments has been closed and the fate of the collection is unknown. A research Institute founded at Mathura by R. D. Gupta for collecting and preserving local manuscripts is run on a shoestring. Even the holdings of Indian material in famous European libraries are largely uncatalogued.

5. K. R. Popper, "On the Sources of Knowledge and of Ignorance," in *Conjectures and Refutations* (London: Routledge and Kegan Paul, 1963), pp. 3-30.

6. Professor Karl W. Deutsch pointed out to me that collections of data are still important for testing theories. I certainly accept this *caveat* particularly in relation to the social sciences.

7. Richard F. Gombrich, *Precept and Practice, Traditional Buddhism in the Rural Highlands of Ceylon* (Oxford: Clarendon Press, 1971).

8. *In Search of Cultural History* (Oxford: Clarendon Press, 1969).

9. For this and the following see also my "Plea for Pluralism" in *The American Art Journal*, III, 1 (Spring 1971), pp. 83-87.

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NORTHROP FRYE

The Search for Acceptable Words

I HAD GRAVE DOUBTS about my fitness to discuss the question of research in the humanities, because I have been deflected from everything that could conventionally be described as research, in the sense of reading material that other people have not read, or have read for a different purpose. The reason why I take an autobiographical line in what follows, even at the risk of sounding egocentric, is that my experiences as a scholar have seemed to me, for a long time, to be atypical, and that more recently I have begun to think that they may be more typical than I have suspected.

Towards the end of the nineteenth century, the system of Ph.D. training was established in American universities, largely, it is said, on a German model. The idea of the Ph.D. was to define the condition that a practicing scholar was supposed to keep himself in. His thesis was there to demonstrate that he had the capacity to make original contributions to scholarship, and the rest of the training was to prepare him to teach over a wider field than his special scholarship covered, and to be able to make use of research materials in other languages. Scholarship in the humanities was conceived at that time largely in terms of the division of labor, and the model, implicitly or explicitly, was science. The scholar was by definition a specialist, connected with his community through his teaching. The implication was that the highest reward of the scholar would be relief from teaching duties, at least in the undergraduate school, so that he could devote his time to his speciality. Behind this lay a further assumption about the place of the university in the community at large. Because of its innate tendency to build smaller compartments in knowledge, research gives an illusion of withdrawing from social issues, hence a hierarchy of scholarship in which pure research is at the top also assumes a university that constitutes a social counter-environment.

At this time and later a number of learned journals were established, many of them with the word "philology" in their titles, which indicated that the scholarly organization of literature had been derived from the comparative study of languages. And though the Ph.D. took a much longer time to establish itself in the British universities, especially Oxford, where I studied during the thirties, still Oxford had also adopted the philological conception of literature. There were three courses in the English school at Oxford when I was there: course one,

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which had the highest rating, featured Gothic and Old Norse; course two covered English literature down to 1500; course three, established for the benefit of Rhodes scholars and the like, covered English literature down to 1830. Whatever the course, the emphasis was on philological elements, rather than on literary or critical ones. On both sides of the Atlantic it has been relatively recently that we have had a sustained body of scholarship in the Old and Middle English periods that is not critically infantile.

The excuse offered for this was that, as any cultivated person ought to be reading literature on his own, giving academic credit for doing so was a weak permissiveness. There are colleges in Oxford which to this day refuse to employ a tutor in English for this reason. It is part of the same perverted ethos that what is regarded as serious in the study of the modern languages is the philological aspect of it. It strengthens the moral fibre to learn the classes of Old English strong verbs; it weakens it to study the rhetorical devices in *Ulysses*. Here again is a lurking analogy to science, and when nineteenth-century philology gave place to twentieth-century linguistics, some of the old prejudices against literature as such revived.

A dissatisfaction with this ethos reached its climax around the forties of the century in America, when a movement described as "new criticism," because it was new to the *Wissenschaft* mentality, raised a question that was almost a moral one. Why should people be regarded as scholars in the area of literature who did not know the first thing about literature? For the first thing about literature to know is how to read a poem as a poem and not as a philological document. In endeavoring to develop this, the new criticism lost itself in a labyrinth of explication. Many aspects of the reaction, as usually happens in such cases, were carried to absurd lengths. A strong undergraduate feeling developed that departments of English in universities were under a moral obligation to be as contemporary as possible, and a similar feeling expressed itself recently under the neo-Nazi slogan of "relevance." There were also graduate students who would propound the thesis that literary works could be properly read only when they were deprived of all context in language, history and experience. In the mean time the old philological journals had begun to lose their prestige and were being replaced by new journals, most of them with "review" in their titles, and which printed poetry and fiction as well as criticism. For all the excesses, the reaction itself was inevitable and in its main emphasis healthy. After a long period of specialized scholarship concerned with literature which had no real basis in literature, a great body of information about literature had been built up which very seldom led to any increased insight into it. Hence there had to be a period of drawing things together, of trying to look at literature itself in a broader and more general way.

It seemed to me, entering this situation a generation ago, that the first thing to look for was a basis for critical principles within criticism itself, trying to avoid the kind of externalized determinism in which criticism has to be "based on" something else, carried around in some kind of religious or Marxist or Freudian wheelchair. At that time, scholars who regarded their work as historical

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assumed that anyone dissatisfied with their methods must be anti-historical. This was the reverse of my own attitude: I was dissatisfied with the methods of historical scholars who did not know any history. That is, who did not know the history of literature. There were many who knew dates and the numbers of the centuries and a certain amount of non-literary history, but who did not know anything about the actual development of the conventions and genres of literature itself. I think that enough theoretical work has been done now to make visible a shift of emphasis, and that we are at the beginning of another phase of scholarship, based more solidly on a properly established critical theory of literature. There may again be some specialization and division of labor, but the old pseudo-scientific analogy has had it. I should be very pleased if I were to become regarded as one of the people who had assisted in the process of transition.

It is true that I have also suggested that criticism may become a scientific activity. But the conception of science involved is different. The older conception rested on a work-and-play antithesis: philology was the one way of working with words, so far as was then known, which seemed to have affinities with scientific procedure. Consequently philology was work, whereas literature, to which only a purely emotional reaction was thought of as appropriate, escaped every form of systematic and progressive study. I have tried to show, or help show, that literature itself is a structure, and can be studied in sequence like anything else; hence for me the entire study of it can assume a scientific shape. I am thinking, of course, of a future development of science in which the social sciences will have rediscovered the fact that they are equidistant from the humanities and the physical sciences, and are as closely related to the former as to the latter.

The transition however leaves many problems unsolved, notably the question of the Ph.D. The humanities resist the division of labor much more actively than the sciences, and the collapse of the scientific analogy in graduate work in the humanities puts this degree in a curiously paradoxical position. In the humanities, things stick together, get involved with one another, and merge into larger configurated patterns. Consequently, the graduate student's Ph.D. thesis is almost always going to be his first book. So, obviously, he has to get the thesis done, because if he doesn't it will block up everything else he might do. It is possible to pick a thesis topic which is a pure academic exercise, and can be done in a limited time, but such topics are rare, and many of them are exercises in which the student will learn nothing except the technique of the exercise. Much more frequently, he gets discouraged by the terrible waste of time and effort involved in writing first a dissertation and then a book on the same material.

There are many things in the Ph.D. program which are extremely valuable, as I know to my cost. I avoided the Ph.D. myself by sheer accident, but there were elements in the training which I wish I had got in the regular way, and have always felt the lack of. Some efforts have been made to put in additional degrees in place of the Ph.D., with very limited success. To put in a degree that is simply an inferior degree seems to me entirely useless. A few years ago,

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some graduate departments at Toronto, including English, experimented with an M. Phil. degree, but soon abandoned it. I am afraid too that they also abandoned most of the students ill-advised enough to take it. The attempt was part of a move to get qualified teachers into classrooms in a hurry during a period of rapid expansion. But periods of rapid expansion in the university never last: depression is the university's normal state, and second-class degrees merely supply a pretext for cutting staff in harder times.

The same thing is true, I should imagine, of degrees regarded as theoretically equivalent to the Ph.D., but with emphasis on teaching rather than research. In the new conception of the university's place in the community which is now emerging, teaching and research cannot be separated even in emphasis. If research is subordinated to teaching, the instructor soon falls behind in his subject, and his teaching suffers accordingly. If teaching is subordinated to research, the instructor, unless he leaves the university and attaches himself to a research institute, loses touch with the social context of his research. I understand that in the University of London a doctoral degree is awarded on the completion of a certain body of work, but is independent of graduate study as such. This seems to me to make very good sense for the humanities. But in general I really have no solution to the doctoral problem in literature, and can only offer one or two observations about the nature of the academic area which may be of some use.

I notice that whenever I publish an article and get offprints, I may send the offprints to friends, but I seldom get any requests for them. On the other hand, when I gave an address to a convention of psychiatrists that was printed in a psychiatric journal, I got over a hundred requests for offprints. It was no surprise to me to learn that scientists tend to work with offprints and abstracts, but I had not realized before so strongly how much the humanist tends to wait for the book. It is as though the humanist cannot really understand any aspect of his subject unless he studies a large configuration of it.

The book is a by-product of the art of writing, and is the technological instrument that makes democracy a working possibility. The expository treatise, in particular, is a democratic form in which the writer is putting all his cards on the table, avoiding all rhetorical tricks designed to induce hypnosis in an audience, relying on nothing but the inner force and continuity of his argument. The reader cannot directly reply, but he can always turn back in the book to a previous point, and find that the same words are repeated no matter how often they are consulted, a model of patience for the teacher in itself. Behind the book is the larger social context of a body of written documents to which there is public access, the guarantee of the fairness of that internal debate on which democracy rests.

The authority established here goes back to the fateful point in Greek philosophy represented by Socrates. Students of Pythagoras or Heraclitus were expected to be disciples, pondering the dark sayings like "change is a rest," "all things flow," "don't eat beans," and the like, which had the unquestioned authority of a guru or oral teacher. Then we have Socrates approaching the youth of Athens and saying, in effect: "I don't know anything, but I'm looking

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for something. Come and help me look." Those who responded found themselves pursuing a straight line of dialectic, an argument which had its own authority and autonomy independent of the teacher. Socrates was not a writer, but his linear habit of thought made him an inspiration to writers, beginning with Plato and Xenophon. Socrates was also a philosopher, and philosophy has, up until quite recent times, also been based on the book. The centrifugal drift towards scientific procedures has affected philosophy as well, with, possibly, though I speak with very little authority, the same result. I remember as a student of the subject having to read, for example, Lotze's *Mikrokosmus*. I have totally forgotten what Lotze's philosophical "position" was, or into what ocean of thought the delta of his argument debouched itself. But I do remember feeling that I was reading a wise and humane book, which had got to be that through the relaxed and comprehensive form adopted. The shape of the book, in short, has a great deal to do with the liberality of the discipline that produces it.

I am on a commission in Canada concerned with communications, and when I first joined it I read a policy report which recommended that publications should be issued from time to time. The recommendation began: "Despite the disadvantages inherent in the linear representation of a world that is increasingly simultaneous, print still retains its medieval authority." This sentence is typical of the nit-witted McLuhanism (I am not speaking of McLuhan himself) which is confusing the educational scene. I recently spoke to an audience of university graduates, and was asked, quite seriously, what I thought of the university's building such a huge library when the book would be out of date by the time it was opened. The book *qua* book is not linear: we follow a line while we are reading it, but the book itself is a stationary visual focus of a community. It is the electronic media that increase the amount of linear experience, of things seen and heard that are as quickly forgotten. One sees the effects on students: a superficial alertness combined with increased difficulty in preserving the intellectual continuity that is the chief characteristic of education.

I mentioned the fact that scholarship tends to become pluralistic and increasingly specialized, increasingly unintelligible even to its nearest neighbors. The core of truth in the older conception of the scholarly life, which placed pure research at the top of a hierarchy, is that scholarship always holds a potential power of veto over everything else. That is, the tiniest alteration of established fact may have repercussions that will totally change the generally held view of the whole subject. But the scholar remains connected with his community as a teacher, as a public figure, and as a popularizer (in the best sense, naturally) of his own subject. This is the level for which the book is the inevitable form of presentation. Perhaps, as remarked above, we may expect in the humanities a new crop of research articles and special studies which can be produced after new theoretical principles have been established. I gather from talking to scientists that they seem to feel that they are at the other end of the cycle, and that all the sciences, even the physical ones, are feeling a greater need for books and for larger and more comprehensive patterns of thought.

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The book raises the question of the nature of influence in literature. This is one aspect of literary study that has received a good deal of attention, because its basis appears to be historical. But scholars trained in extra-literary perception, interested in the history of ideas and the like, are apt to think of an influence transmitted from A to B as a large body of consciously held ideas. This is hardly the way that influence exerts itself among poets. One may assume, for example, that Blake, Shelley, Keats and Wordsworth would all have made an intensive study of Milton. But what they got from Milton they got as poets, and consequently the derivation is a mainly unconscious derivation of phrases, even words, that flow from a single source into four very different contexts. Probably the best way to document the influence of Milton on these poets is to run their texts through a computer: the result would not, in general, tell us much that we could not already guess, but the documentation would be useful.

The computer has made, or can make, other changes in the structure of literary study. A hundred years ago, even fifty years ago, there was a strong existential reason for emphasizing values in the study of literature. It was one thing to decide to commit a large part of your life to making a concordance to Shakespeare or to collating manuscript variants of Chaucer, but would you want to do that kind of work for medieval homilies, or for the kind of poetic achievement represented by Googe or Churchyard? The computer has not only altered the answer to the question, but has helped to erode the fallacy of hierarchical values behind it. When Professor Douglas Bush produced his book on Classical mythology in the Renaissance a generation ago, he tells us that he had made an immense number of observations about the treatment of some Ovidian myths in minor Elizabethan poets. He regarded this work as largely a waste of time, and remarked that even though he knew more than anyone else about such subjects he should keep his information to himself. I suspect that this information was not really too trivial to be passed on, but I think it may indicate an area where some kind of mechanical aid might be of assistance. Whatever was highly regarded in its day, such as the Pyramus and Thisbe legend, is of great importance merely for that reason, whatever a contemporary scholar may think of it.

But there is another type of influence that it is impossible to trace except by an occasional accident. Every creative person has an interconnected body of images and ideas underneath his consciousness which it is his creative work to fish up in bits and pieces. Sometimes a phrase or a word comes to him as a kind of hook or bait with which to catch something that he knows is down there. Reading Yeats, one would think that he owed a large-scale debt to the writings of Villiers de l'Isle Adam, and to *Axël* in particular, which would be a ready-made subject for the scholar to explicate. On examination, we find that Yeats had been fascinated by a single phrase, "As for living, our servants will do that for us," extracted, totally out of its context, from a play he could hardly understand in the original French, and applied in contexts even more remote. Poets are full of influences of this kind, vagrant seeds blown toward a responsive soil, and not only poets.

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When one reads the studies of influence made on creative people by their teachers, by factors in their early environment, by their reading, one is usually struck by the great plausibility of these constructs: one does, in fact, become what one is through influences. At the same time we should remember that it almost certainly did not happen like this at all. Anyone reading *Finnegans Wake* thinks he can see at a glance that *Alice Through the Looking-Glass* was a major formative influence on it, especially the figure of Humpty Dumpty and his portmanteau words. Yet we learn that Joyce had not in fact known what was in that book until it was called to his attention by others. Again, one can see that of all writers in the past, the one closest to Joyce in both temperament and technique is Rabelais, and one could study the parallels between them exhaustively. Yet Joyce says that he had not read Rabelais, though he expects nobody to believe him.

Even in my own work I can occasionally trace the same process of transmission by seed. When I began teaching, the University of Toronto possessed an honor course in English Language and Literature (now destroyed, in a fit of hysterical exuberance, because it was said to be "elitist"), which was spread over four colleges, each of which had its own department. This meant that I not only had to teach Milton, but teach Milton opposite Professors A. S. P. Woodhouse and A. E. Barker, two of the best Milton scholars anywhere, who were teaching it in the other colleges. One result of this was that for several years I confined my reading to primary sources, there being no time to read secondary ones. Later on, when the pressure slackened a bit, I attempted to adopt the normal routine of checking through learned journals. I soon cut down on that activity, not because I regarded the articles as useless, but for the opposite reason: I was interested in everything, everything seemed to have some relevance to my interests, and yet the pursuit of knowledge in all directions at once was impracticable. Ever since then, I have realized that scholarship is as much a matter of knowing what not to read as of knowing what to read. While writing *Anatomy of Criticism* in particular, endless tantalizing vistas opened up on all sides, yet I had to close my eyes to them, as Ulysses closed his ears to the sirens, because exploring them would get my main thesis out of proportion.

Fortunately, one of my colleagues when I began teaching was Professor Wilson Knight, later of Leeds. I think Wilson Knight influenced me more than I realized at the time. At that time he was completely possessed by Shakespeare, and gave the impression of not knowing a Quarto from a Folio text, certainly of caring even less. He showed me once his main instrument of scholarship—a Globe Shakespeare with a mass of pencilled annotations. Like most students of my generation, Knight's books had much the effect on me that Chapman's Homer had on Keats, and the method indicated, of concentrating on the author's text but recreating it by studying the structure of imagery and metaphor, seemed to me then, and seems to me still, the sort of thing that criticism is centrally about.

Nevertheless, I went through a long period in which every publication of mine was followed by neurotic fears of being confronted with proof of having plagiarized it from some source I had not read—or, worse still, had forgotten hav-

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ing read. I gradually became more fatalistic about this, besides realizing that the more obvious what I said seemed to me, the less likely it was that anyone had said it before. I can also trace one or two examples of influence so trivial that I hesitate to record them, except as evidence that the kind of influence I postulate for Joyce and Yeats works on all levels of intellectual activity. Once, when an undergraduate, I was discussing the keyboard music of Bach with a friend interested in the same subject, and spoke of my great interest in the fugues Bach had made on themes by Albinoni. My friend made some such comment as "very scholarly," and the word "scholarly" as applied to Bach's music stuck in my mind. I had always known that it was scholarly, of course, but to know a thing is not to realize it: one may know many things that are still not attached to that submarine body of ideas one is trying to fish up. Forty-odd years later, I wrote an essay on Shakespearean comedy trying to show that Shakespeare is as scholarly a writer as Ben Jonson, except that his scholarship, being connected with the oral tradition, is harder to recognize as such. The analogies with Bach's music are there also, so that here is one example of a seed that I have accidentally caught in the act of germinating.

To carry this point a step further: my first big project was a book on the interpretation of William Blake's longer and more didactic poems, generally called "Prophecies." There were many reasons for getting interested in Blake: perhaps one may be of general interest. I am, in cultural background, what is known as a WASP, and thus belong to the only group in society which it is entirely safe to ridicule. I expected that a good deal of contemporary literature would be devoted to attacking the alleged complacency of the values and standards I had been brought up in, and was not greatly disturbed when it did. But with the rise of Hitler in Germany, the agony of the Spanish Civil War, and the massacres and deportations of Stalinism, things began to get more serious. For Eliot to announce that he was Classical in literature, royalist in politics, and Anglo-Catholic in religion was all part of the game. But the feeling of personal outrage and betrayal that I felt when I opened *After Strange Gods* was something else again. And when Eliot was accompanied by Pound's admiration for Mussolini, Yeats' flirtations with the most irresponsible of the Irish leaders, Wyndham Lewis' interest in Hitler, and the callow Marxism of younger writers, I felt that I could hardly get interested in any poet who was not closer to being the opposite in all respects to what Eliot thought he was. Or, if that was too specific, at least a poet who, even if dead, was still fighting for something that was alive.

When I began work on Blake, around 1933, there was one serious book available on the subject of the symbolism of the Prophecies, Foster Damon's book, which had appeared in 1924. Apart from this there had been hundreds of books, articles, and essays on Blake, to say nothing of thousands of incidental references. There was nothing in any of this material, so far as I know, and I know more about it than most people, which was of the slightest use to me. No device of "information retrieval" would be of any help: the important thing was to get rid of the alleged information, not to get hold of it. And yet, many years later, I was asked to write a bibliographical essay on Blake which was in fact a history of

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Blake criticism. For it, I had to go over this material again, and I discovered, to my great surprise, that it was a most fascinating and rewarding exercise. The books still told me nothing about Blake, but they told me a great deal about the history of taste in the nineteenth and early twentieth centuries. More generally, they told me a great deal about the human ability to stare at what is straight in front of one's eyes and not be able to focus one's eyes on it. I know now much that I could hardly have learned in any other way about the anxieties and obsessions which prevent a great writer from being properly estimated in his day and for some time after. I note from a reprint catalogue that four of these early books have recently been reissued. As far as telling us anything about Blake is concerned, all these books are trash, and one of them would be a strong contender in the admittedly stiff competition for the title of the world's worst book on Blake. But in a sense, you can't lose in the humanities: if your book is any good, it's a contribution to scholarship; if it's no good, it's a document in the history of taste.

Two morals seem to me to be relevant. First, it is a part of the fallacy of the scientific analogy that, before deciding on a thesis topic, one should look to see whether or not "it's already been done." Of course this did not apply to my Blake project. But in general, in literary criticism, nothing can ever be done in a definitive way, except very specific projects, such as editing texts. I have supervised a good many doctoral theses on Joyce, Yeats, Stevens, and other much-processed writers, and have been well aware that all these theses were saying very similar things. But each represented an individual point of view, and this kind of individuality has to be taken account of in the humanities when we speak of a contribution to knowledge. In this situation the "review of recent scholarship" article emerges as an indispensable scholarly tool.

Second, a principle which follows from the first, the question of personal authority is relevant to the humanities in general, and literary criticism in particular. I think it advisable for every critic proposing to devote his life to literary scholarship to pick a major writer of literature as a kind of spiritual preceptor for himself, whatever the subject of his thesis. I am not speaking, of course, of any sort of moral model, but it seems to me that growing up inside a mind so large that one has no sense of claustophobia within it is an irreplaceable experience in humane studies. Some kind of transmission by seed goes on here too. I am venturing on an area which so far as I know has been very little discussed, and what I say is bound to be tentative. Keats remarks that the life of a man of genius is a continuous allegory, which I take to mean, among other things, that a creative life has something to do with choosing a life-style. I think the scholarly life has something to do with this too, and one chooses a preceptor among the poets who has something congenial to oneself in this respect. I notice that, at the age of sixty, I have unconsciously arranged my life so that nothing has ever happened to me, and no biographer could possibly take the smallest interest in me. The reason for this unconscious choice is that, for me, an obliteration of incident was necessary to keep the sense of continuity in the memory that fostered the germinating process I have spoken of. And it is clear to me, though not demonstrable to anyone else, that this has been imitated, on a level that conscious-

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ness and memory cannot reach, from Blake, who similarly obliterated incident in his own life, and for similar reasons. One who found Byron more congenial as a preceptor would doubtless adopt a different life-style.

Blake, while he is very much a man of his time, tends to pull his critic out of the historical period in which he falls. So while I naturally had to become familiar with Ossian and Gray and Chatterton, I also found myself looking through a great variety of other writers in search of ideas that would give me some clue to Blake. I do not mean that I read very systematically in mystical or occult literature. Most of the misguided critics I mentioned above told me that that was where the real analogues to Blake's thinking were. But I found it hard to understand why Blake interested me so much when most mystical and occult writing interested me not at all. I soon discovered that I had to take seriously Blake's own statement that he belonged with the poets and artists, and read accordingly. But again, reading simply in the 1750-1820 period of Blake's own lifetime was not only insufficient, but in many respects misleading. This fact was important to me, because it dramatized the difficulty of knowing what not to read, which I have already alluded to. The historical period has firmly established itself as the normal area of scholarship because it both limits and directs one's reading, and in nearly every case it is a safe guide to follow. Blake was an exception, and a very significant one.

When I was taking a course in Blake from the late Herbert Davis, I was assigned a paper on Blake's *Milton,* which I sat down to write, as was my regular bad habit in those days, the night before. The foreground of the paper was commentary, which was assuredly difficult enough for that poem, but in the background there was some principle that kept eluding me. On inspection, the principle seemed to be that Milton and Blake were connected by their use of the Bible, which was not merely commonplace but seemed anti-literary as well. If Milton and Blake were alike on this point, that likeness merely concealed what was individual about each of them, so that in pursuing the likeness I was chasing a shadow and avoiding the substance. Around about three in the morning a different kind of intuition hit me, though it took me twenty years to articulate it. The two poets were connected by the *same* thing, and sameness leads to individual variety, just as likeness leads to monotony. I began dimly to see that the principle pulling me away from the historical period was the principle of mythological framework. The Bible had provided a frame of mythology for European poets: an immense number of critical problems began to solve themselves as soon as one realized this.

Further, Biblical mythology had not remained static, but had grown with a catholicity greater than that of the Church itself, annexing the whole of Classical mythology, and the erotic or "Courtly Love" literature as well, as contrapuntal descants on its own theme. The fact that one mythology could absorb another indicated that all mythologies were imaginatively much alike. Christianity had similarly absorbed the older Teutonic mythology, because the latter also had a world tree like that of Eden, a world-girdling serpent like Leviathan, a god hung on a tree as a sacrifice to himself, a last day and a creation. A literature grows out

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of the primitive verbal culture which contains a mythology; it can grow out of any mythology, but it is a historical fact that our literature is most directly descended from the Biblical myth.

Of course this particular discovery was a natural one for me to make at a time when I was actually a student of theology. This latter fact has proved useful to many people. A "Maoist" pamphlet, for example, describes me as "the High Priest of clerical obscurantism," and its cover depicts a series of black-cloaked monks, with hoods suggesting the Ku Klux Klan. I think the actual effect of my theological training has been rather the opposite of that. It is true that my attitude to teaching, and probably to scholarship as well, has always been an evangelical attitude, and that I was not satisfied with my own theories until I began to see how they could be made the basis for a system of teaching literature in sequence, at all levels from kindergarten to graduate school. But actually the Bible preoccupied me, not because it represented a religious "position" congenial to my own, but for the opposite reason. It illustrated the imaginative assumptions on which Western poets had proceeded; consequently the study of it pointed the way towards a phenomenological criticism which would be as far as possible free of presuppositions. I am not by any means sure that it is possible really to get free of presuppositions, but it is obvious that all genuine advance in knowledge goes along with a continued attempt to objectify and become aware of the assumptions one is starting from. Poets do not write, like Swift's spider, "out of their own bowels, and in a restricted compass." The poet is taken over by a mythical and metaphorical organism, with its historical roots in the Bible, and the integrity of that organism is his Muse, the mother that brings to life a being separate both from herself and from him.

So far from hitching literature to a structure of belief, this principle actually emancipates literature from questions of belief altogether. But, of course, for many centuries the poet was regarded as subordinate in authority to other types of writers, such as theologians. Their method of writing, it was thought, had more direct access to truth; the poet's function was to produce a rhetorical echo of that truth, addressing the emotions and will and trying to persuade them to align themselves on the side of truth. This view assumed that literature was serious in proportion to its allegorical relationship to religion. Later on, as religious anxieties gave place to more secular and political ones, it continued to be believed that the realist, who studied the life around him and reflected it in his writing, had a seriousness that the mere romancer who told stories for fun could not match. The historian of nineteenth century fiction, for example, finds that the backbone of his historical study is constituted by the great realists, George Eliot, Jane Austen, Thackeray, and certain carefully selected aspects of Scott, Dickens and Henry James. Such a book as *Alice in Wonderland* is obviously a masterpiece of its kind, was immensely popular in its day and has never lost its popularity, but somehow it doesn't fit the history. And just as a highbrow in Old English times would probably regard a saint's life as more serious than *Beowulf*, so most modern critics would regard social realism, of the kind that clearly reflects the life around it, as more serious than fantasy or adventure, where there is a

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strong emphasis on the structure of the story, designed primarily to keep the reader turning the pages.

This leads to another far-reaching critical principle. Every primitive verbal culture contains a number of stories, of which some gradually assume a particular importance as "true," or in some way more deeply significant. These are the stories that are most readily describable as myths, and they are the ones that take root in a specific society and provide for that society a network of shared allusion and experience. Such myths differ in social function, but not in structure, from the folk tales and legends that are often told simply for fun by wandering story-tellers. Thus in European literature the Biblical stories have a seriousness not ascribed to folk tales or legends which are closely analogous, if not identical, in structure. As literature develops, some poets, such as Dante and Milton, recreate the central myths; others, such as Shakespeare, have the social role of entertainers, and turn rather to folk tale and legend. But the former group recreate the central myths on the same imaginative basis that the latter are using, and the latter group, if they treat their themes with enough intensity, give them the same kind of significance that the central myths have.

Some time ago I had occasion to read Katherine Anne Porter's *Ship of Fools*. It is not a favorite novel of mine, but it struggles hard to be a serious one, and it seeks seriousness through allegory, like the Sebastian Brant poem from which it derives its name. The setting is a German ship sailing from Mexico to Germany in 1932, and every episode, such as the excluding of a Jew from the captain's table, is part of an allegorical relationship of the story to the rise of Nazism in Germany. Later, when stranded in an airport, I pulled another ship story off the paperback shelves, Paul Gallico's *The Poseidon Adventure*. This was a thriller about a ship that turned upside down in a wreck, leaving the surviving passengers to climb upwards towards the bottom of the ship. This story being designed for entertainment, it concentrated on the shape of the story, and the shape of the story derives from what I call an archetype, a story type that has been used thousands of times before, for which the Biblical model is the Exodus. The author being a professional and intelligent writer, I knew that he would indicate some awareness of this fact before long, and two-thirds of the way through he did so. He explained that the passengers were undertaking this desperate and apparently futile climb because up had always been good and down bad, that damnation went down, the resurrection up, that mythology puts monsters underground and graceful fantasies of light in the upper air. In the same chapter the leader of the expedition, a clergyman, gets fed up with his own notion of God and commits suicide: not a very convincing episode, but an Exodus story needs a Moses figure who doesn't make it all the way.

I am not comparing the two books: I am noting the survival of the distinction between a more serious literature that reflects important truths allegorically, and a less serious literature, designed for entertainment, that follows a certain story-telling formula. I am also suggesting that an *a priori* value distinction between them is critically unsound. The reading of a formulaic book, such as a detective story, may not be in itself a significant experience. But even such a book can

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take on significance through the resonance of context. A detective story is normally a comedy, and while any individual comedy, from Plautus to modern television, may be silly or trifling, the importance of studying comedy itself, as a whole, is very considerable. For one thing, once a literal belief in a mythology declines, and Biblical stories are no longer generally assumed to be historically factual, it becomes increasingly obvious that the real affinities of a mythology are not with the waking world but with the dream world. A mythology is a construct belonging to art and not to nature: it is not a description of the outer world, a crude form of philosophy or science, but a cultural model, expressing the way in which man wants to shape and reshape the civilization he himself has made. Comedy, which tends toward the victory of desire over reality, indicates this fact more clearly than any other archetypal form.

The same principle is central to the teaching of literature, especially in the most elementary stages. From kindergarten onwards, the teacher is not instilling literature into a mind that doesn't know any, but reshaping the student's total verbal experience. This experience has been built up by television, movies, and the conversation of his parents and classmates as well as by his reading. It already contains a great deal of mythology, much of it phony, derived from advertising or class stereotypes, and literature is or should be the means of leading the student from his present subjective social vision into the total social vision of mankind.

All this may not be very helpful in discussing the question of the university's support of research. The humanities usually get the lion's share of the library budget, but apart from that they are usually regarded by administrators as low-budget departments. In fact there are times when I wonder whether "low-budget departments" would not be the best definition of the humanities. Some branches of the humanities, of course, are inherently expensive, an obvious example being archaeology, on which a good many humane disciplines are heavily dependent. But it is still possible to bring a man into a university of a senior level in the humanities without the great expenditure for equipment and the like which an equivalent appointment in the sciences would necessitate.

Universities however are caught in a paradoxical situation: they have to build up as good a library as they can, and yet they must maintain travel grants to enable scholars in the humanities to go to still better libraries. The immense resources of modern libraries, which can bring so much to the scholar's doorstep, are of course indispensable, but most of them imply that the scholar already knows what he is looking for. If he is doing original research, he may not know. In that case he has still to be turned loose in the British Museum or the Library of Congress with a sense of serendipity built up by his previous experience of the subject.

We have all heard how the Bodleian Library in Oxford got rid of its Shakespeare Folio, which it later had to buy back again at much greater expense. The inference is often that the Oxford librarians of that day were fools. Actually, they were entirely right: in terms of what Oxford then taught and studied, Shakespeare was no more use to them than collections of nineteenth-century ser-

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mons would be now to the kind of college that hands out diplomas to young women studying ballet. After the modern languages were established in universities, library buying was largely confined to a select canon of approved writers. The minor writers came in on a preferential list, those that had been dead the longest getting the most attention. The scholarly revolution which I have helped to agitate has resulted in weakening the distinction between classical and popular literature. Some years ago, when I was visiting a university in the United States, a professor responsible for much of the library buying in the Romantic period came in to see me with a catalogue. He had a chance of buying a large amount of the "horrid mysteries" of the 1790's, and was hesitating. I said, "Grab it at once." But he was doubtful whether the inherent value of this work justified the expenditure, because his was a small college with a restricted budget. I imagine that he now regrets his caution, in an age when "Gothic is in" and the best and brightest graduate students are likely to be paying attention to Rider Haggard as well as to George Eliot. For university library purposes, there certainly is such a thing as junk, but junk is no longer definable in terms of a conventional standard of literary values.

The problem of preserving the personal community is crucial to all universities, and to all aspects of it. One should always be aware of the limitations of what is technically possible, because the future that is technically possible is not necessarily the future that society wants or can absorb. One is constantly hearing from gadget-happy technocrats about how it is, or soon will be, no longer necessary to give lectures or attend them, to build libraries or staff them, to write papers or deliver them to audiences. What such fantasies do not take in is the importance of community and personal contact in the scholarly life. Any student faced by a television screen instead of a human countenance could tell them about this.

It is for the same reason that all educational hardware has to be confined to certain specialized uses. Similarly, many libraries acquire a great deal of material that is really archival, and should perhaps be in another institution. This would include the foul papers and other excreta of contemporary poets, for which there was such a vogue a few years ago, and which for some poets made the faking of draft papers quite a profitable business. In my own case, I have had to throw my energy into a more centripetal movement of scholarship, trying to avoid specialization in order to articulate a number of central problems of critical theory. This meant that a specialized research library was only intermittently useful to me, and I have often found myself drifting from the research library into the undergraduate working library, because of the increasing affinity of my work to obvious books. But there is something too of finding one's way back to the focus of the university community. When I mention this tendency of mine to my colleagues, I find that a large number of them share it.

I have always remained in Canada, and perhaps the influences of the Canadian environment have played a significant role in my life, allowing for the indirectness of influence that I have already mentioned. I think in any case that the effect of one's teachers and senior colleagues is derived mainly from the reflec-

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tions that one is impelled to make as a result of having known them, rather than from what they have directly taught or said. My own teachers in Toronto included Pelham Edgar, whose main academic interest was Henry James and James' treatment of the North Atlantic schizophrenia which is so central to Canadian life also. Then there was E. J. Pratt, Canada's most important English poet, who was a full-time teaching member of the department. His existence helped me to become more detached from the romantic mystique that opposes creative writers to critical ones. In Canada poets are conditioned to utter a good deal of anti-academic patter as a part of their own sales pitch, even when they are struggling for tenure appointments in universities themselves. The mystique tends to assume that creative people are the people who write poems or stories, and feels that such people ought to have a specially protected place in the community, somewhat analogous to that of the people who can speak Irish in Ireland. There is nothing wrong with this except the fallacy of attaching the conception of creativity to the genres of poetry and fiction rather than to the people working in them. This fallacy is world wide, and extends, for instance, to the Nobel Prize Committee in Sweden, who search over the world for poets and novelists but never consider the claims of critical or expository writing. My third teacher, John Robins, was interested in ballads, folk tales, and popular literature, along with Old and Middle English. He had much to do with my understanding of the main features of Canadian literature, including Pratt's poetry, which are so like those of Old English in reflecting a paradoxical tension between the primitive and the over-civilized. He probably had something to do too with my notion of archetypes, which are really an expansion of the themes and motifs of folk tale into the rest of literature.

Later on, I began to understand the extent to which this almost one-dimensional country has been preoccupied with communications of all kinds, from the most physical to the most ethereal. Many Canadians, including Harold Innes and Innes' disciple Marshall McLuhan, have been interested in the totality of communication, and the essential unity of its activity, whether it is building railways or sending messages. Innes developed out of his study of the fur trade in Canada a vast historical communication theory, concerned with the ways in which the control of communication media by a certain class or group in society conditions that society. A similar sense of the unity of communication has affected me, and has had a good deal to do with what I have called my evangelical attitude to the teaching of literature.

The first thing one realizes, of course, is that communication media are formidably efficient, and the effect of this is among other things to make every senior professor a cock-shy of rapid transit. He has his growing body of former students, who send him everything they write as a simple routine act; colleagues who ask his advice about everything from changing jobs or making appointments to rearranging the arguments of their current books; publishers who keep sending new books to him for comment; chairmen of program committees who have found that jet planes make it possible for anyone to visit anywhere, and for conferences to be held in Pakistan or Brazil as easily as closer to home. There

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is also a continuous cataract of unsolicited material, stacks of song from hopeful poets and theses with notes attached saying in effect: "My supervisor says I'm crazy, but I know *you'll* understand." In this situation, an enlightened university administration should understand that the most important form of assistance they can offer him is that of the non-academic secretary. Such a secretary can make all the difference between a properly functioning scholar and a harried intellectual carpet-bagger, and he should not have to undertake an uncongenial administrative job in order to acquire one.

There seem to me to be three main stages in communication. There is the initial stage of separation, where communication is physically difficult and precarious. This developed in Canadian culture what I have elsewhere called a "garrison mentality," and which survives in Canadian separatist movements. The garrison mentality however had its positive, even its heroic, side, as we can see when we think of the sheer physical, to say nothing of the mental, effort that scholars in Saskatchewan or Alberta had to make to maintain their standards before the days of planes and microfilm. The first impact of improved communication is destructive. If one is building a road from point A to point B, the first thing one has to do is bulldoze and cut through underbrush. If a student is being taught at school, the effort to reach him has to cut through the underbrush of the anxieties and prejudices and snobberies by which he attempts to maintain his isolated security. The immediate result of this destructive activity is a sense of uniformity and a loss of individuality. This is the stage that the communication crisis of the sixties is trying to emerge from now.

Similar stages have affected the universities internally. Each department tends to become a garrison in itself; every development of scholarship batters at its barriers. My own conception of mythology is one that attacks the separation of one language department from another, the separation of literature from comparative religion, even the separation of the humanities from the social sciences. One response to this is to introduce interdisciplinary courses, where representatives of departments meet politely but suspiciously, like diplomats arranging for a cease-fire, or courses in general education or general humanities or great books or western civilization. This is the second, or uniform, stage of improved communications, which is less a solution to the problem than a symptom of it. One hopes for rapid emergence to the third stage, when it is clearly realized that knowledge, like St. Augustine's God, has its center everywhere and its circumference nowhere.

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ERIC WEIL

Supporting the Humanities

THE PRESENT WRITER will have to start with an admission: he is convinced of the value of the humanities (to be precise, of history and philosophy). Even more, he is convinced that only the humanities are able to handle the question of the worth of human occupations—including the sciences proper—because the sciences are unable to reflect upon questions which bear on man considered as an acting, thinking, feeling subject, not as an object for analytic, factorial, causal analysis. He knows quite well that he is up against a mightly phalanx representing the opposing view and against its Goliath, old Thorstein Veblen, who, in his nearly unbearably repetitious manner, made bold to show that there is no legitimate place for traditional learning in modern society: the humanities, he claimed, "are, from the point of view of economic efficiency in the broader sense, disserviceable anachronisms," and the classical languages are "substantially useless information."[1] This contention is formidable indeed, since Western, and even Eastern societies insofar as they are to be considered "modern," have gone over to Veblen, wiped out "anthropomorphic," "priestly," and "leisure class" ideals, put a stigma on concepts of "status" and "envious comparison," and renounced everything non-productive and not "workmanlike." What Veblen called the study of causal relations in nature and society has become, it would appear, the central interest in our world.

At least it seems so. Perhaps a change is becoming perceptible precisely in the place where Veblenism first exercised a strong influence—in the United States. A great deal of the student unrest, widespread throughout institutions of higher learning, had its origin and found expression in domains that may be called humanistic. Behind the unrest, however, was not so much a questioning of facts and methods, but of ends and meanings. And, surprisingly enough from Veblen's point of view, it happened in institutions open to criticism on many accounts, but certainly not for their lack of interest in "causal" sciences, natural or social. Moreover, many of these institutions could not possibly be considered the preserve of the scions of the leisure class, but were filled with the offspring of the modern, industrial groups. I do not know to what extent the outlook of American students is changing (very legitimate material interests intervene in young people's choices of future careers); but a new interest in classical, "non-scientific," humanistic philosophy is gaining momentum

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among younger teachers and researchers, and problems and subjects formerly taboo among respectable people in the universities are making their appearance in "serious" journals. Sometimes one even gets the feeling that the public at large is not always sure that Veblen's ideal of workmanship and of production for production's sake is enough.

The situation is not the same in Europe, probably because the movement toward the elimination of the "useless" started later in this part of the world and thus has, on the one hand, a greater life expectancy, while on the other, it must compete with a still surviving tradition, not to mention the insufficiencies in technical teaching sometimes defended with pseudo-humanistic or pseudo-humanitarian arguments. It would not be foolhardy to assert that the different *reformes* of the French university system are simply moves on the way to transforming the universities into higher technical schools. No doubt nobody wants to throw out the humanities: we owe that much respect to our glorious past. But what use can we make of so many people educated in history, philosophy, classical studies, and literature? Some of them are needed in our secondary schools, although fewer and fewer since such subjects play a smaller and smaller part in these schools. Their task ought to be—and probably will be—to instruct useful and usable young people for the service of a community given to production and consumption—a consumption which, by the way, would have appeared to Veblen purely ostentatious.

Even in France certain young (and not so young) people have begun to doubt the value of this conception, and again their problems as well as their muddle-headed, often simply stupid answers fall under the heading of the humanities, not under that of the sciences. Although this activist fringe (or vanguard?) would not necessarily accept the suggestion that they are looking for a historical and philosophical understanding in any academic sense, they will nevertheless have to look in these domains for answers if their answers are not to be arbitrary, violently destructive, and totally unconvincing in the eyes of the majorities (and the minorities) which make the decisions. Their actions are very often simply inadmissible; but the reaction which leads to these manifestations is very understandable: a life in meaningless comfort is a meaningless life.

The indications are that nowhere can instruction alone replace education; by itself it does not form human beings capable of giving a sense to their existence except in blind adhesion to the moral and political quack, a kind of prophet whose number has become legion and whose success most often originates in appeals to half-decayed traditions which, half-forgotten though they are, continue to be transmitted through never explicitly formulated moral or aesthetic rules, attitudes, images, exemplary types.

II

It may be convenient to look at the place of leisure in our society in a way Veblen himself would have considered "scientific." The part of a human being's

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lifetime given to production has dwindled rapidly and goes on diminishing. Nobody now enters the production process at twelve or thirteen years; the working hours and the working week's upper limit have gone down and continue to do so; workers and employees in France at present take four weeks' vacation; and the retirement age has been made lower and lower. It suffices to compare these figures with those obtaining in Veblen's time to see that leisure has become one of the most important products of industrial civilization. It would be only mildly risky to say that we are moving toward a time when useful employment will be in short supply. Our political leaders are talking more and more of the need to create new jobs or to spread out existing ones by means of instituting shorter working hours, later entry, earlier retirement. There once was, if we accept Veblen's thinking, a leisure class; it looks as if we are developing a civilization of general leisure.

Even according to Veblenian principles there is always a place for unworkmanlike activities, especially for those of the humanities, in a society which unavoidably produces leisure. In a rather unVeblenian spirit it should be added that these activities are not necessarily mere games and sports. It would not be a bad thing if citizens made their decisions not because this or that candidate looked pleasant, but because they understood, if not all the technical problems, at least the central issues and could eliminate the obscuring sales talk; if men and women did not follow quacks and cranks, but chose instead among ideals that might be, if not completely realized, at least approached; if they could distinguish historical factors from mythical explanations; and if the people at the helm looked at what in the past had led to success or failure (or had been the undoing of the actors); if moral and even aesthetic judgments were not confused with private preferences and personal tastes; and if the thinkers or artists were not lumped together with sincere madmen and authentic speculators counting on the public's ignorance and on its fear of missing the last bus.

But does this objective need for thought (pleonastically, humanistic thought) correspond to a demand? Do the people in the market place or in the universities ask for more thinking, or has the movement come full circle? Has the bad name which, according to Veblen, was once attached to the sciences now been inherited by the humanities, once considered "noble" because useless, now suspect because they have remained useless in terms of production? And if the humanities are nevertheless useful and necessary to the search for a meaningful and, by consequence, satisfying and non-violent life, what, exactly, can they do? What ought they to do? What are they doing?

III

It will be necessary to look first at the facts—the way the humanities are being pursued in terms of both teaching and research. Undoubtedly the humanities are not disappearing from our curricula. The reasons our universities give for maintaining the humanities often have a dubious effect on the studies

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themselves. We maintain them because they form part of what we call our cultural inheritance; at the same time we consider it necessary that we ourselves and our pupils be capable of understanding our historical situation so that we can orient ourselves and help them to do so. Thus we are always tempted to limit our field to our national cultural inheritance—our national literature, the history of our nation, the development of our particular sort of thinking and our national philosophy. Most historians (there are great, even admirable exceptions) do their work in a somewhat narrow frame. French or English or American history is being written and, above all, taught, as if purely national lines of development actually existed and every foreign influence were just a disturbance, instead of an essential part of a single history in which all the threads contribute to the web of a single historical world. If it is only a question of teaching, this curriculum can be justified to a certain extent; but research will always be the fountainhead of any education which aspires to do more than make people learn by rote dead facts, and here the answer will be very different.

No examples are needed, for each of us has only to take cognizance of the situation in his own domain. But there is more to it. It must be added that, as another consequence of our organizational setup, the different chapters of the humanities do not form a book. Everybody is afraid of sticking out his neck and correspondingly lives on the air he finds under his carapace. People do history of philosophy and philosophy of history without giving much time to history. Moreover, ideas are retraced without relation to the conditions that, while they did not produce them (conditions never produce ideas), created the receptive atmosphere indispensable to their hearing and their survival. Possibly it will be objected that to pursue the end of a history without lacunae is a dream, since no teacher would be qualified to teach it and no student of sufficiently capacious intellect to receive it. But this criticism starts precisely from the misunderstanding of education here under scrutiny. Education is not a question of cramming, but of discovering and showing lines and problems and an orientation; the cultivated man is not he who can quote the contents of an encyclopaedia, but he who knows what to look for and where to look for it. If research and teaching are not conceived in this spirit, we get the well-known result of boredom and disgust.

IV

Institutions of research and higher learning are inherently conservative. It is not in their nature, however, to become ossified and opposed to change. Rather they are conservative in the sense of the man who wants to preserve what in his eyes is valuable and who, for that very reason, is prepared to accept and introduce what is required to avoid violent destruction or the dying off of what he cherishes.

In the sciences as well as in the humanities transmission and renewal must be reconciled. But the sciences have constituted theories that, at least in principle, are consistent and coherent. There is, always ideally speaking, a consen-

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sus, a community recognized by the participants in the common, although strongly subdivided, enterprise as qualified to pronounce not only on results, but on the interest of research projects. This (ideal) solidarity enables them to indicate unsolved problems that deserve tackling and to acknowledge the interest and value of new perspectives. Exceptions there are—as, for instance, Galois' revolutionary concepts in mathematics, which have been acted on only a long time after his death. We have no *a priori* certitude that other such cases may not be put at our doorstep by future scientific theorists. Nevertheless, exceptions remain exceptions, and an exaggerated fear of future reproaches should not lead us to accept what to the best of our knowledge we can consider only cranky, incompetent, and without scientific interest. The accumulation from revaluations and revolutions gives meaning to the concept of progress in the sciences (and in technology which is based on science and often contributes, to the development of "pure"—that is, not immediately applicable—theory).

The situation of the humanities is at the same time exactly the same and basically different. It is the same as far as erudition—the discovery of facts and the methodical analysis of factual relations—is the object. That the humanities are concerned with far more than mere erudition, I hope I have made clear. But this is not to imply that erudition is superfluous or destructive of the spirit of the humanities. True enough, erudition, if it is nothing but erudition, is dry and dries out the mind, if at the start there was a mind and not simply a technical intelligence. But understanding and the effort to discover a meaning cannot possibly proceed without access to the documents, to what authors have actually written as opposed to the slipshod copying and printing of their work. Our results would be faulty were we to rely, since nobody knows all languages, on negligent translations, if we had no access to texts surviving only in manuscripts or extremely rare prints, manuscripts and prints which are not always easy to read and must be transliterated in order to become useful for people not all of whom are necessarily paleographers or epigraphists. Erudition is an indispensable prerequisite to this process, and although the error is extremely popular, it remains a mistake to renounce the necessary conditions under the pretext that they are not the sufficient ones. It follows that there is accumulation, revision, and progress in the humanities as well as in the sciences. But enrichment in the humanities is of a different kind since research does not produce ideas (although unconsciously it depends on them: nobody would publish a text which he did not consider, at least potentially, to be interesting—interesting in its relation to an idea). Humanistic research is the accumulation of tools for work of another order than its own.

Here lies part of the task of the institutions and the people who decide how the funds at their disposal should be used, for what concrete projects they are to be employed or collected. But the dreary fact is that the consensus which exists in the sciences to a large extent seems difficult to discover or to establish in the humanities. The absence of such a (more or less) tacit agreement makes it no easy job to decide which line of research to further, which to drop. An additional difficulty results because the humanities have no relation to

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productive technology. A research chemist who discovers only that he is unable to discover anything of interest will take a job in industry for which he is probably highly qualified; on the other hand, because he has few alternatives to the academy, a young man who has opted for the humanities is, as a rule, forced to persevere in his labor although he may have come to hate what he is doing (often letting out his spleen on his students). Graver still, the humanities do not receive the incitements that, for instance, the fields of applied or applicable physiology or ecology do from the narrow public of direct users and the wide one of the people at large constantly indicating general trends and common desires. And not only that: the academic institutions, which for these reasons are the home of the humanities and practically the only home for the humanistic student, are not necessarily favorable to large perspectives, particularly where collaboration would be desirable between specialists and departments traditionally living at a certain distance. Many an interesting thesis has not been written simply because it was too difficult to decide who was to supervise it. (It would be more than useful to admit that a piece of work could be done under the auspices of different specialists—which, of course, would only be possible if such specialists were prepared to discuss the subject among themselves. This departmentalization makes for the neglect of whole lines of research which do not belong to a well-defined domain or are not of evident importance within its limits. Lastly, the students, or the great majority among them, are not generally interested in research; they normally and legitimately look first for information about things they have not learned in high school and their teachers are compelled to take care of these needs first. As a consequence, the heads of departments will concentrate, in their pedagogical plans and their recruiting policies, not on problems related to research and scientific progress, but on the desires, the attitudes, and the expectations of a public opinion in whose eyes the humanities remain just an ornament, not something serious. This public subscribes to a somewhat liberal kind of Veblenism, the signs of change we mentioned revealing only new trends, not a new power and pressure distribution.

V

Under these conditions it seems clear that the allocation of credits and personnel devoted to scientific progress in the humanities ought to come from authorities that are not involved in the fighting interests of departmentalized and mainly pedagogical humanistic teaching. The moot point is that a university is but a collection of departments, and that research councils, such as the Centre National de la Recherche Scientifique in France or the big American foundations, have need of advisers. (They can also be under the sway of political authorities who, as in France, acknowledge with every sign of respect the autonomy of teaching and research and keep in their hands only the overall budget and the determination of what part of it will go to—Veblenishly—

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interesting research and what is left, if anything, to the humanities.) These advisers are to be found mainly, not to say exclusively, in the universities, with the result that inter-departmental strife continues in the research councils, only on a higher level—each group fighting for a maximum of credits. Thus no overall planning exists and only projects in particular fields are considered, after which stage the financial authorities cut back still further. Under these circumstances, any work interesting in a wider perspective is not even considered, much less initiated. We are going around in circles: the only people who could change the situation are prevented from doing so because they depend on people who, as a rule, have no general views and do not even want to hear of such things.

One or two instances may be useful. There is, for example, Hegel's *Philosophy of Right,* one of the most influential books in the history of political thought and action; Marx as well as the father of German conservatism, von Stahl, although they are both critical of the work, become understandable only in relation to it. Now, there are good reasons to believe that Hegel wrote his book in 1819-20 because he wanted to influence the discussions on the future constitution of Prussia which were going on in a Royal Commission. These discussions are not inaccessible, nor is Hegel's text, and it would be invaluable to know exactly the political intentions of Hegel's teaching for his and the following time. But philosophers will say that the work ought to be done by a historian, and historians will shift the load toward the philosopher, both declaring with becoming modesty that the subject is beyond their range.

One may take as well the case of Neo-Stoicism. The present writer is convinced that this school of thought played a major, even a central part, during the sixteenth and seventeenth centuries, and hardly a negligible one in the eighteenth. Descartes, Spinoza, Calvin (who began his career with a commentary to one of Seneca's writings)—the line could be traced back to Montaigne and forward to Kant—cannot really be interpreted without a knowledge of this tradition; it follows that these authors are credited with an originality they themselves would not have admitted, simply because we do not study what every cultured man in their times had always present in his mind. But again philosophers will say that such research belongs to people interested in neo-Latin literature (there are not many of them, that matter certainly not being a part of the standard curriculum) and the Latinists will refuse because they do not want to meddle with philosophy, or theology, or literature. Nobody seems to worry over this blank spot on our intellectual map. And if none of the present reprint houses thinks it can make money out of these sources, the institutions of higher learning, the foundations, and the university presses will certainly not make the necessary texts accessible. Even less will they take the initiative in proposing such subjects. But people might be tempted to undertake the work if they were invited to do it and were sure of receiving the required leisure—that is the financial backing.

To sum up, much could be done on the institutional level; much could be done in international collaboration, the way it is occasionally by UNESCO and

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by a small number of learned journals (but too often under pressure for arithmetical international equilibrium and compromise). Yet it could be done and ought to be done.

VI

Erudition is necessary, and we could get it with well distributed money—for books, for leisure, for some apparatus. But the end, to underline it once more, remains understanding—understanding of ourselves, who are the heirs to ways of thought and living we are, for the most part, unable to seize and who therefore fall victim to what in another field is called unconscious repression.

Now, looking for the hidden meaning of this past which dominates our present, for the real meaning of what we carry with us without reflecting on it, is not a task undertaken by those colleges and universities which consider themselves mainly as teaching institutions, as transmitters of humanistic erudition. This does not signify that as such they are superfluous ornaments; the contrary is true in a civilization producing mass leisure, a civilization where people have to look more and more for something to replace work. It is, of course, possible to throw the job to those who do not refuse to make a profit out of it. Industrial tourism, thrilling television shows, moving and exciting novels and periodicals, alcohol, drugs, sex rationally organized and exploited are at our disposal if we want to kill time and to commit suicide by installments. In fact, we have to solve only the second half of the Roman problem of procuring *panem et circenses* for the masses: the *panis* there is, at least for the overwhelming majority, since those whom we term poor would have been regarded as leading a scandalously luxurious life by a worker, and not only by a worker, a hundred or a hundred and fifty years ago; what we are in need of and are consuming more and more of is the *circenses*. The pertinent question is whether the entertainments we are currently offered are good enough. Do they deliver the goods required? Here some doubt is permissible. One of the characteristics of our time seems to be the appearance of gratuitious violence, not the sort of violence all epochs have known—the violence of need—but disinterested violence, the most radical expression of the most radical boredom.

The immediate consequence may possibly be considered as rather queer by many respectable people. It is this: universities, while going on with the indispensable work of instructing and of intellectually disciplining the young, ought not to take entertainment as their central task; rather, they should try to become places where grown adult men and women can discuss among themselves and with adult specialists what occupies their minds, and occupies it, at present, without that clarity which results from dialogue, dialogue being the way in which the unconscious and the half-conscious come to the surface and can be tackled. A substantial advantage could even accrue to the academic community from such contacts, since living more or less completely in the society of the very young often keeps the teacher's mind in a state of juvenility not altogether desirable. Teachers would be tempted to put into perspective

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what they are too often teaching mechanically. Conversely, the adults would benefit from contacts with young people whose naive blindness to what understanding is, and simple ignorance can be extremely instructive for an adult mind, always in danger of considering as evident what in fact it has acquired only after long effort and with great pains. So much the better if that happens and if the humanist is thus enabled to establish both a living contact with the young and an authentic discussion with adults from the outside, thus gaining a feeling for the desires and needs of his time—something very different from, but equivalent to, the impetus the sciences receive from the demands of economic society. We need adults, and adults are more than simply humans past biological puberty: ripeness is all. But the humanities alone can educate people for being adult, and this education will always be the work of adults—adults knowing that they have to live with problems which will never be solved for good. Moral disquisitions (and *moral* is only another word for *humanistic*) are not for the very young, as Aristotle said, adding however that youth and adulthood are not dependent on birth dates; or, to invoke another authority, a young sage is better than an old fool.

The problem is to produce (or to maintain where it exists) a culture for everybody who feels the need for something better than mere distraction, a culture for the masses, to use the ugly formula, but a culture which transforms masses into thinking and responsible men and women. Starting, but only starting, with boys and girls, it would develop persons and personalities able to unravel what obsesses them and pushes them toward violence, to *feel* instead of following instincts, to look with understanding at models of human perfection and at typical situations of conflict and tragedy, to gain self-reliance and self-respect. The real task and the very justification of the humanities—and of the universities—lie there.

VII

What are the universities and the foundations to do about it? To state it bluntly, they distribute money—money necessary for what the humanist needs: leisure, books, and (marginally) a certain amount of apparatus, such as secretarial help, use of computers, aid in publishing, altogether negligible requirements compared to those of the scientist. The problem resides where we already said it did: in coordination and planning.

In principle it ought to be easy, and it is, so long as it remains a question of establishing facts, looking for documents, editing texts, preparing dictionaries, and so on. But this kind of work, as we know only too well, can easily be technically competent and totally irrelevant. Where are the criteria of relevance? Again we meet the trouble specific to the humanities: we cannot plan here as we are able to do in areas where we know at least our trouble spots and can say what problem is most urgent or most interesting. The same difficulty persistently returns; we have not got that explicit or implicit consensus which prevails among scientists.

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This is because fundamental questions in the humanities originate in our situation as it appears to us here and now. The surprising changes in the preference given at different times to different historical periods—to certain ways of feeling and thinking—are sufficient proof of a basic fact: we are always trying to understand ourselves. We project *our* problems onto the men and situations of the past and choose from the past so that the dialogue with its actors may help us in our own predicament through contact with something at the same time different and familiar. Now, although all that is past is different, not all of it always seems familiar: Virgil was a guiding star for a long time; now he has become an object of research, but he may once again become a model for poets, once again furnish new generations with human types of extreme importance to them. There is nothing in the human past that could not be revived; but in each resurrection everything is changed because the eyes looking at the past are not the same. In this sense, there is neither accumulation nor loss; there also is no progress and no rational planning. The new in the humanities very often, not to say regularly, comes as a shock and a scandal, an effect no revolutionary deed in the sciences seems to have provoked since science has become "modern"; and even the revolution which introduced this modernity was a scandal not for the scientists of the time, but only for those who looked at its consequences for religion and for the place of man in the universe—that is, for the humanists. For astronomers and physicists, Copernicus was not a danger, but a man who indicated ways and means for solving riddles which had occupied their minds a long time.

But there is more. In the humanities every new perspective becomes part of what the humanities are fundamentally interested in. A "discovery" does not just drop out or become part of the common stock of knowledge, but gives rise to new problems. Erudite history is perfectly able to make out if an alleged fact took place and when; but history is not a simple recital of facts. The historian cannot avoid distinguishing between the important and the unimportant. He follows, without reflecting upon them, certain values, certain lines, a certain overall interpretation, and different historians will follow different lines. It is no accident, therefore, that the history of history has become a very respectable undertaking which in due course may lead to a history of the history of historiography. In any case, both the history of history and the history of philosophy are legitimate subjects for philosophers. It is a problem for systematic philosophy to determine whether this reflection on reflection is infinite; fortunately we are not obliged to tackle that question here. But reflection on what is unexamined and reflection upon reflection exist and the fact is in itself of the highest importance for the way research and teaching are to be financed or directed. Here, as always, he who pays the piper calls the tune, especially when the traditional scholar, the gentleman of independent means (and means sufficient to the technical necessities of the present) has disappeared. This problem of direction is not easy, especially when everything new appears to later times naive and unexamined, and when what a first reflection reveals as naive and unexamined may yet gain through a second level reflection new life and new importance.

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The number of "returns to . . . ," returns to Plato, to Kant, to Marx, to Clausewitz, to Burckhardt, prove this observation abundantly. It is always the present that we are interpreting, but we interpret it by looking not at *the* past, but what we choose as *our* past, and we do it with the help of those who have preceded us, of those we choose as *our* predecessors among the inexhaustible riches at our disposal. Everything gets stale if it is simply handed down, and everything comes alive if we take it on with the feeling of *tua res agitur*. We do not know from the start what matters for us, we are trying to find out just that in the unceasing reflective dialogue of the humanities. The fundamental trouble is here, since ultimately the question is one of new approaches, not simply of solving old riddles or eliminating more or less well-known difficulties.

VIII

What, in the final analysis, can foundations, universities, and ministries of public instruction do? Quite a lot. They can—or, should one say, could?—help to prepare tools for the humanist, an area where planning is both possible and to be wished for. A coordination, for instance, in the choice of subjects for doctoral dissertations could prevent useless efforts by eliminating themes which have the advantage only of being easily managed under personal and local conditions or of a bibliography not too rich; multiple editions of texts might be avoided, and work on previously unedited sources encouraged. Research could be directed by indicating where specialized work would fill gaps which beginners under serious supervision would be perfectly able to close. The institutions could do much—and the point is particularly important—if they encouraged interdepartmental research and, to begin with, interdepartmental instruction and education. They could think seriously about the part they could, and therefore should, play in and for society as a whole, not only—not even principally—by looking for what is to be done in order to bring the lawyers, teachers, and doctors up to the newest standards, but by admitting and encouraging and inviting people in search of a humane existence to come into contact with one another and with our cultural tradition, a tradition whose very presence remains hidden to many of them. They will only come, of course, if, owing to stupid cramming, they have not already come to hate the humanities.

All that requires money if research is to continue and to advance, not lots of money in comparison to what must be spent elsewhere, but money nevertheless. Unfortunately money is not enough; something more is required, a kind of daring which intervenes as well in scientific research, but to a lesser degree (although the financial risk is incomparably greater). As has been pointed out before, there is in science a consensus, actual or potential, about what is needed and where the open problems lie. The situation is profoundly different in the humanities. Physicists before Einstein knew that something was wrong with Newtonian space, although they were unable to tell what the trouble was or how it could be eliminated. On the other hand, nobody could have predicted what was to come out of research concerning the unconscious mind before Freud

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started to look at it and so opened perspectives nobody had dreamed of—with results that many consider regrettable and others appreciate as highly valuable. In any case Freud's work constitutes a decisive event in our historical, moral and political life; we are not what we were before. Similar examples could be made into a very long list: Marx's view of history, or abstract painting, or modern poetry, or new philosophies—none of them could have been predicted or even asked for. It is no objection that they can be historically understood: of course they can, but only once they have appeared. Originality in the humanities does not have the same meaning it has in the sciences, simply because original work in the humanities not only changes the direction of research, but changes us.

The ground layer, the greatness, and the difficulty of the humanities appear when they are viewed in their nature—as a human and humane and humanizing study. This nature creates severe difficulty for those who have to make decisions at the organizational level: Nobody can say beforehand if a new idea will change the world or if it will be judged a crank's obsession. The temptation is great to look at what is new with distrust because nobody wants to have his fundamental attitude questioned, be it that of his personal life or of his professional work. New perspectives may, and will, render obsolete and uninteresting a man's conceptions and his research. (This is temporary, however, since nothing is ever definitively lost in the humanities.) It could be said that the troubles of the present, our common present, are there to guide us by showing us what is wrong; but that is true only when troubles are defined as our feelings of discontent, and the problem is precisely to transform these feelings into thoughts.

There is much, very much, to do. Our present perspectives are the only ones at hand: we cannot just stand and wait for something new and original to come up. We live and act not tomorrow, but today, and we must pursue and finance and organize whatever seems to be our task at this moment in time, whatever seems to be obtainable here and now for the people we presently are. But we must be open—the institutions must be open—for what we are not accustomed to and what, as a rule, will shock us. We have to take risks, and the institutions have to play the part played a long time ago by Maecenas, without whose intervention we would never have known Virgil or Horace, and who probably, before he found Virgil and Horace, spent large amounts of money on people whom we have never heard of because they did not deserve to survive.

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JOSEPH BEN-DAVID

How to Organize Research in the Social Sciences*

WRITING ABOUT the organization of research assumes, as a rule, the knowledge of what research is, and what research is about. Such knowledge cannot be taken for granted in the social sciences. In this field there is usually a gap, and often a very considerable gap between the theoretical description of what is being done and what is actually being done, and there is widespread feeling that neither is very satisfactory.[1] Therefore, one cannot proceed to talk about the organization of research before one tries to say something about research itself in the social sciences.

The reason for misunderstandings concerning social science research is that social scientists formed their conception of natural science on the basis of the descriptions of philosophers of science or of philosophically inclined scientists. These descriptions are not necessarily inaccurate, but they are selective. On the basis of these descriptions it is, perhaps, justifiable to identify science with physics. That discipline comes closer than any other to producing very general, yet precise and testable theories; it can, therefore, be considered the clearest example of what scientific thought is, in principle, capable of accomplishing.[2] Consequently, social science, this most recent of all the sciences, which deals with phenomena of the greatest complexity, tries to model itself on the oldest of the natural sciences, physics, which deals with phenomena of the least complexity.[3] The worst effects of this misunderstanding can be discerned in so-called social science theory. There is an assumption that social science theory has to have a very high degree of generality, like, presumably, physics theory. Since to aspire to such generality is completely out of tune with the empirical inquiries of social scientists, what actually happens is that social scientists present empirical approaches as if they were general theories. Examples of this abound in sociology. Due to the complexity of social life it is relatively easy in this field to find things

* Social science in this paper includes anthropology, sociology, political science, personality and social psychology, and history. The problems of economics and experimental psychology are different from those of the fields above, and will be referred to only occasionally. I am indebted to Professor Edward Shils for several valuable suggestions, and to the Ford Foundation which supported my stay at the University of Chicago where this paper was written.

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which no one has ever before examined scientifically, or to view well known processes from a new angle. One may look at social processes taking place in a group from the point of view of how labor is divided; or of what strategies are employed by the participants in the establishment of their identities; or of how conditions of equilibrium are established; or of how conflicts develop with other groups. Instead of trying to relate these different points of view to each other, sociologists have a widespread tendency to misrepresent each viewpoint as a new theory. Since in most cases neither the old nor the new approaches are scientific theories, but, at the best, rationales for the sociologist's interest in various aspects of social life, differences of opinion among sociologists can never be resolved.[4] As a result, the dogmatism and in-fighting between adherents of different approaches can continue forever without ever being of any benefit to knowledge.

When the methodology of the natural sciences is introduced into the social sciences, a somewhat different problem arises. What social scientists do is not, in the majority of cases, an experiment. To give an everyday example, a sociologist may be interested in the differences in the morale of teachers at various schools. The schools he has a chance to investigate will probably be located in a certain place (city, country, etc.) at a certain time. However, since morale is probably determined by a great variety of conditions, it is still quite possible that in each school a different set of conditions determines it. Therefore, finding an explanation will depend on the researcher's having a good initial idea of the whole range of *possible* conditions affecting morale, and on his ability to construct a working typology of the various causal processes affecting morale within those particular schools and school systems to which he has access. For instance, the salaries of the teachers—which are probably one of the conditions affecting morale—are determined by economic processes. But the subjective meaning of a certain salary, which must be understood to gauge its effect on morale, depends on social psychological reference group processes. These processes are, in turn, largely determined by certain characteristics of the school and school system, by the nature of the leadership in the school (i.e., the way the headmaster performs his functions), and by the characteristics of the community within which the school is situated. The better the sociologist understands the mechanisms of these basic processes, the less likely he will be to make a mistake. But in no case can he explain the whole situation from his knowledge of basic underlying processes and their interrelationship. He must relate these processes to particular events and to the peculiar conditions of social structure and culture prevailing in a particular place and time. Therefore he must start with a more or less empirically grounded and partly intuitive explanatory model and then check it constantly both against empirical evidence and against his improving knowledge of the underlying processes and structural regularities. The social scientist investigating this kind of problem ought to proceed in an eclectic manner, using whatever theories serve him, irrespective of their disciplinary provenance. The proper model for this kind of work is clinical medicine or engineering, each of which uses a variety of disciplines for its purposes.[5] The social science researcher

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ought to regard himself as a re-constructor of social structures and processes, working on the borderline of science and literature, as the clinician or engineer works on the borderline of science and art.

But because sociologists are trying to follow the model of the basic natural sciences they tend to force problems into disciplinary frameworks and to treat them according to their relevance to some theory as if the purpose of the investigation were to test hypotheses. The results are usually trivial from the point of view of theory, and much too abstract to be useful in practice.

Of course, this kind of concrete investigation is not all that social scientists do. Like clinical or engineering scientists (as distinct from practitioners), they also engage in basic research. But this basic research does not stand in the same relation to their everyday work as physics, say, stands to the practice of many branches of chemistry. Although some sociologists, like George C. Homans and W. G. Runciman, believe that there must be a general psychological theory from which all sociological explanations (except, of course, those which are contingent on historical circumstances) can be derived,[6] such a theory has not yet emerged, and my guess is that it is unlikely to do so in the near future.

Therefore basic research in social science is something vague and variable. There is no single basic science, and what is considered as the most important of the several basic sciences in a given field changes often, like fashion. Twenty years ago all branches of psychology, with the partial exception of experimental animal psychology, were considered important basic sciences for sociology. Today, there is much less interest in psychology among sociologists, and what there is, is mainly in experimental animal psychology. In the forties and the fifties mathematically trained sociologists pressed for the development of statistical methods especially designed for sociology, and considered econometrics irrelevant for sociologists. Today the trend is almost completely reversed and the tendency is to consider econometric methods and economic theory as basic to sociology.[7]

But the most typical kind of basic research, especially in anthropology, sociology, and political science, is some kind of comparative research. Theories of electoral behavior, or theories governing marriage patterns among different kinds of kinship groups (to the extent that one can speak of them as theories) have to be tested by comparative studies.

Such studies range from those, in demography for instance, which lead to theories of very high generality to those, such as comparative studies of the electoral behavior of first voters, which differ only in scope and emphasis from the type of study undertaken by clinical medicine or engineering. Some of this research (e.g., demography) may fit the model of the most advanced natural sciences. But usually it resembles the work of the geologist, or the evolutionary geneticist, rather than that of the physicist, for it consists first of the gathering and proper classification of observations from all parts of the world and of all historical periods, and then of fitting them into evolutionary, or structural-functional, models. And in this work too, social scientists will have to make recourse to a variety of theories from even more "basic" fields.

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II

According to my analysis, one of the principal problems in social science is this lack of recognition that many of the questions investigated by social scientists require an eclectic approach like that of clinical medicine or engineering. In this respect there seems actually to have been some retrogression since the investigation of social problems carried out in the United States during the 1930's. Projects such as *Recent Social Trends* published in 1934 as a result of the collaboration of outstanding economists, political scientists, and sociologists, or the work on racial discrimination in the U.S.A., directed by Gunnar Myrdal, an economist working with sociologists, did not have a sequel after World War II.[8]

Starting perhaps with the studies on the American soldier, and on the authoritarian personality,[9] social scientists tended to try and combine practical advice with theoretically significant contributions to one or another of the social science disciplines. Eventually, they began to consider that the contribution to the discipline was more important than the practical conclusions.[10] This led to a unidisciplinary approach. Instead of asking where the knowledge to solve a practical problem could be found, researchers broke down the problem into its various aspects—sociological, economic, etc.—and were content to handle only those aspects which concerned their specialty. Because of their conception that social sciences had to resemble the basic natural sciences, they saw this development as an improvement in the status and quality of the social sciences.

Recently there has been a change of attitude towards science, and now the model of applied science is favored over that of basic science. But this change of attitude has not been accompanied by a re-conceptualization of the whole problem of social science research. Before they can organize their research, social scientists must address themselves to the question of whether the aim of their research is, on the one hand, to establish generally valid rules or principles, or, on the other hand, to explain particular events with the aid of general principles. If it is the latter, as I maintain it very often is, then this kind of research is in its logical structure a clinical-engineering type research whether its findings are ever practically applied or not.

However, social scientists do not accept this model as suitable for their research. This is because, on the whole, social science has been used for the diffuse purpose of "enlightenment"; namely for the interpreting of social situations and social objectives rather than for formulating detailed strategies for the attainment of those objectives. What attempts there have been to use social research in a technically precise way for the design of practical social action have usually been short-lived and unsuccessful.[11] One reason that there is little interest in the clinical-engineering model is that it is identified with these unsuccessful attempts to solve practical problems.

This, of course, is no reason to reject the clinical-engineering model, since, as I have pointed out, the appropriateness of that model does not depend on the uses made of the research, but on the nature of the question which is investigated. To the extent that the question involves the interpretation of a particular event,

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rather than the general rule underlying many events, the clinical-engineering model is appropriate regardless of whether the results are used for social engineering, or for mere enlightenment.

Nevertheless, it seems useful to explore to what extent social scientists today see their function as that of enlightenment. What is called "enlightenment" includes several kinds of functions. The one which comes closest to what is ordinarily meant by enlightenment consists of comment on current affairs by scholars engaged in basic, or, at any rate, non-practical research. The comment is journalistic in form, and does not pretend to be anything else, but it benefits from the fact that the commentator is first and foremost a scholar, and that he confines himself to writing on problems related to his scholarship. This kind of work is practiced with great success by people like Raymond Aron, Milton Friedman and Paul Samuelson.

But enlightenment is not the most typical function of social sciences today. More commonly social scientists are called in to consult on specific programs, or to conduct research in order to use the results in specific programs. Even research not done with a specific practical aim in mind is often used, after it is finished, for specific action purposes. Thus the recommendations of the Robbins Committee served as the basis of official policy toward higher education in England,[12] and various policies adopted in the United States for combatting discrimination, such as the busing of school children and the Head Start program, have been directly linked to social science research.[13] Ideas of participatory democracy advocated by the New Left can be linked to the practices of group dynamics initiated by the students of Kurt Lewin. And the recent permissiveness of the courts, of the legal profession, and of the general public towards criminals is based on the view that criminality is the result of social pathology, so that dealing with the individual criminal does not go to the roots of the problem. This view derives from sociological and psychiatric research on the roots of crime. The utopian ideas which have become attached to these research results are usually far beyond what the investigators would have considered legitimate applications of their ideas.

All of this is evidence that although social scientists are rarely in charge of action programs, their work has become an integral part of social therapy and social engineering designs. But, because the role of the social researcher in the utilization of his findings is not clearly defined, this utilization is often premature, misguided or even utopian. There is little or no feedback from practical experience to research, so that researchers rarely have the opportunity to work gradual improvements. Typically, a social science idea or result is taken up by policy-makers with much enthusiasm but little understanding, and used in a way not justified by the actual findings. The failures which are then encountered are blamed on the inadequacy of social science.

Furthermore, many social scientists—within and outside the academic framework—are engaged in research directly linked to action programs; this has become their principal occupation, rather than a by-product of their scholarly pursuits. As a result, the pertinence and quality of their practical

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contributions are no longer guaranteed by the quality of their scholarly work, and must be judged instead by criteria intrinsic to the purpose of their work, a purpose as precisely practical in many cases, as those in clinical work or engineering. Thus, the adoption of a clinical-engineering model in the social sciences is required not only because it is an appropriate model for social science research in general, but also because, in practice, social science research is often pursuing action-oriented goals similar to those in clinical medicine or engineering.

These circumstances lend a particular urgency to the matter. Unless we can create conditions to effectively control, test and follow up situations in which the findings of social science research are applied to practical purposes, social science research will be misused and exploited—deliberately or with good will—by policy makers, journalists, political groups and anyone else interested in social novelties. Social scientists themselves may be corrupted into becoming entrepreneurs in social novelties, or quacks and propagandists of political utopias. Therefore, the question is not whether the clinical-engineering model should be adopted or not, but how can it be adopted most effectively.

III

As I have mentioned, one of the first requirements of this type of research is that it should be open to different disciplinary approaches and theories. But, contrary to general assumptions, this end cannot be furthered very effectively by the organization of inter-disciplinary teams.[14] When work is based on a broad, rather than a narrow view of possible explanations it is important that the people engaged in it have a broad enough background. Then, if it seems useful, they can communicate and cooperate with people from other fields, and perhaps, come to share with them a vital intellectual interest in the solution of a given problem. Today, however, universities do not train such social scientists. The solution to this problem lies in changing the curricula and the methods for training social researchers, the discussion of which would require a separate paper.

A certain amount can be done by organizational means. First, efforts and resources should be redistributed so that the social scientist engaged in explaining a situation (past or present, for practical or for purely scholarly purposes) or in devising a program to solve a social problem, could spend more and more time on diagnosis and design, and less and less on background and preliminary research. At present he must spend most of his time on the latter.

For instance, a local school system may be interested in having a researcher investigate the problem of dropouts. The obvious choice of procedure would be to see which of the known causes for dropping out are present in the situation and then to try out remedies which have worked in the past in similar cases. However, in the absence of reliable information even about "known" causes, not to speak of workable remedies, the researcher will either undertake a local survey based on whatever variables he can find in the literature and thus try to identify causes by multivariate analysis, or he will devise an experimental

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program. Neither is likely to be of great practical use, since what can be learned from any small scale one-time study or experiment is of limited practical value. Nor is such research likely to yield theoretically significant results, since the changes are small that such quasi-practical studies will be so designed as to fill an important gap in knowledge. As a rule they will only confirm what was more or less known already and make recommendations which could have been made before.

All of this contrasts sharply with the work of a clinician diagnosing an illness, or of an engineer designing a machine. Either of these does work which is more precise, more carefully tested, and more innovative than most applied social research, yet he does not undertake anything like the primary and background research undertaken by the social scientist. Were a clinician or an engineer obliged to survey the relevant state of his art for himself and were he expected to test basic hypotheses, his work would not be of much practical use since it would be forbiddingly slow and expensive. Furthermore, the number of doctors and engineers capable and qualified to do this kind of work is extremely limited. Yet this kind of work is precisely what applied social scientists are trying to do.

A first step toward remedying the situation would be to create and maintain a system of information storage and retrieval which would make possible a gradual reduction in primary and background research. In addition to first-rate libraries, such a system would have to include archives of past social research; various series of social statistics; and handbooks containing a guide to these archives and statistical series and a reliable compilation of the data most frequently used in different fields of social research.

Social scientists have to set up archives of information on social research. In most fields there are abstract-journals, and in educational research there are data banks as well. But valuable data used in past research often are destroyed or difficult to obtain, because there are no archives to sift, keep and circulate these materials.

As to series of social statistics, these exist only where they serve the administrative needs of governments, or of other public or private agencies. The social researcher finds their usefulness limited because the agencies which keep them make frequent changes in the definitions of the categories and in administrative rules and practices without regard to his needs.[15] An important beginning has now been made toward remedying this situation in the United States through the attempt to establish so-called "social indicators."[16] Although there will always be indicators left out in any general project, the information on these could gradually be improved by the existence of data archives.

But both archives and social statistics would be of limited use, without the availability of handbooks based on them and, presumably, on additional information as well. It is difficult to imagine a physician without a collection of handbooks on drugs, and one on different categories of diseases; or an engineer working without similar guides in his field. And in the humanities, little philological work could be done without specialized dictionaries, handbooks and similar aids.[17] Only social science is virtually without handbooks.[18]

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The maintenance of archives and statistics, and the editing and updating of handbooks are difficult and thankless jobs. They are not the kinds of tasks which arouse enthusiasm. Among social scientists, in addition, there is a prejudice against large scale collections of data not designed for some specific immediate use; there is also a general suspicion of big science projects involving great numbers of auxiliary personnel.

The prejudice against collecting data for general rather than specific purposes is founded on experience with huge poorly designed surveys which produced material that no one used. The objection to such surveys is justified, but what I am suggesting has nothing to do with those. Rather, what I am suggesting here is that records of the findings of successful past research be maintained and effectively circulated, and that the collection and publication of data generally useful to researchers be placed on a continuous basis. Past attempts at omnibus one-time data collection have often been without guiding concepts; in contrast I am suggesting that we devote many more resources to creating a system whereby the results of well conceived research and data collection can be continuously fed back into ongoing research.

My proposals have nothing in common with certain wasteful and intellectually sterile big research projects. I do not advocate large scale organization of research, but large scale division of labor in research. As a matter of fact, the present situation encourages the growth of big science, since an increasing variety of empirical social research is only possible if the researcher relies on a large survey team or organization. If we had better information retrieval and, above all good handbooks, some of this research could be done by lone researchers, who are still numerous and significant in the social sciences, but who are at an increasing disadvantage, technically, relative to those who work within research organizations.

My suggestions would go a long way to avoid the much lamented loss of past achievements, and to prevent unnecessary new beginnings in social science. But they would not fully resolve the problem of discontinuity in the uses of social research. As I pointed out above, at present social science is used in erratic, unpredictable, and occasionally illegitimate ways, and there is no feedback mechanism to evaluate successes and failures in the application of social science research. Handbooks might improve the situation somewhat by making much of past experience easily accessible, but they could not ensure more responsible use of social science, or more systematic evaluation of its practice.

Changes in this situation can come only from the social scientists themselves. It is difficult to expect the users of social science, who hardly know what it is about, to do anything else but use it according to their convenience. It is up to social scientists to take themselves seriously as professionals, to demand a share first in conceptualizing the problem, then in devising the solution for which their aid is sought, and finally in formulating programs based on their findings. Above all they must insist on participating in the follow up and evaluation of their work.[19]

At present, with very few exceptions, social scientists are not inclined to de-

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fine their practical role in such a consistently professional manner. They are not sure whether their contributions can be specific and practical enough to justify their adoption of such a role. If the tools of their trade were improved in the ways I have recommended, social scientists would be one step closer to the acquisition of greater professional self-confidence.

IV

Another prerequisite to the development of greater professional self-confidence among applied—or for that matter all—social scientists is the improvement of social science theories. Scientific theories which describe and explain regularities in empirically observed events, are as necessary to the professional practitioner as techniques and technical aids.[20] Furthermore, good theories can provide the framework needed for sifting and ordering information in archives or in handbooks, and for designing statistical series which reflect significant (as opposed to trivial) trends. In view of this need for theories, the present trend of demanding that all social science research be "relevant" to the solution of some practical problem is self-defeating. Without great improvement in the basic social sciences, social science research may concern itself with practical problems, but it will rarely produce anything relevant to their solution.

Some parts of basic social science research are probably adequately organized and supported, especially those dealing with statistical methods and mathematical models (although these fields are not adequately utilized because they are poorly taught). But these formal aspects of social science can contribute little to the field in general without systematic comparative research. This is because there are so few possibilities for social experimentation that comparative research is usually the only way to test the validity of a generalization. It is also the only way to obtain the wealth and variety of observations which make theorizing worthwhile and interesting.

In the organization and support of comparative studies there has been actually a retrogression since the 1950's. I have first-hand knowledge of what has happened in sociology, and my impression is that similar developments have taken place in other social sciences. Comparative studies can advance only when competent workers with genuinely common interests can coordinate research in different countries. In the 1950's, when the social sciences, for the first time in their history, became genuinely international, and the International Sociological Association, among others, began working seriously, a number of groups were formed by people sharing an interest in comparative studies. Their work received some small but effective support both from UNESCO and from foundations. The UNESCO funds involved were small, but permanently available. They were allocated rather informally by the ISA which was at that time ruled by a small group of sociologists most of whom were recognized as leading members of the profession. Their criterion for allocating funds to a research committee was that it consist of a strong international group which could be expected to produce good research. The committees were run informally, and

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provided a place where members could meet regularly to exchange ideas, and to discuss informally organized projects. They were, as a rule, productive, especially in the study of social mobility, political sociology, and the sociology of family.[21]

Lately, however, these committees have changed their character. The ISA has become a representative professional body and the committees have been enlarged and democratized. As a result they have ceased to be working groups of active researchers, and became instead bodies representing professional specialties. Consequently, there is no longer any framework for the informal co-ordination of research programs like that which existed during the fifties and early sixties. The disappearance of this framework, combined with the new tendency to pressure social as well as other scientists to do socially relevant research has weakened comparative research considerably. Since practical social policy is made on a national level, the pressure for social relevance usually implies a devaluation of comparative work.

As a result of these developments, a new parochialism has arisen in social research, and support for the kind of social research most likely to improve social theory has virtually disappeared. This trend ought to be, and probably could be, reversed by relatively modest support for comparative research.

V

The argument of this paper is that social scientists have done a disservice to their own work by trying to mold it according to the model of basic natural science. This should not be interpreted as agreement with certain current trends which deny the relevance of scientific logic to the social sciences, or which cast doubt on the validity of scientific logic altogether. However, although the logic of science is generally valid, its proper application differs according to the nature of the problem under investigation.

My main recommendations have been that social scientists should organize their work according to models which suit their own purposes, rather than distort their work to fit the requirements of some ideal (or idealized) models. I have shown that the models which suit social science are those of clinical medicine, engineering, geology and genetics, rather than that of physics. The implication of using these models for the organization of research is that social scientists will have to invest much more seriously in the building of an infrastructure of technical aids to their work; instead of trying to start from scratch in every case, they will have to learn to concentrate on more narrowly defined aspects of a problem, relying on other people's results to fill in the broader picture.

The implication for the social scientist in the definition of his role is that, in those cases where he is actually a social clinician or engineer, he will have to face this fact much more consciously than he has done before. The growth in professional employment (on a permanent or contractural basis) of social scientists has made it difficult to maintain the attitude that the social scientist

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is concerned only with the conception of ideas, and not with the problem of putting them into practice. Not only does the social scientist's lack of concern with the practical applications of his research expose him to blame for the failures of others, but it also deprives him of the benefit of learning from his own mistakes. Here again, I must emphasize that my recommendation has nothing in common with the clamor for politically committed and involved social science. My recommendation is that social scientists should become involved in the objective testing of their ideas as a requirement of their own professional morality, and not that they subject their work to the requirements of an external morality, such as that dictated by a political view.

Finally, my view of social science research implies that much of the distinction between basic science and science applied to practical purposes is irrelevant in social science. The distance between the two kinds of work is so much smaller in the social than in the natural sciences that any attempt at sharply separating the two is completely artificial. Methodologically, work applied to the solution of a present day problem and that applied to the solution of an historical one have a great deal in common. And at this point, all social science is so badly in need of better theoretical knowledge that any tendency to neglect basic work even temporarily, and to concentrate instead on the solution of practical problems can only cause a regression of social science into sets of parochial (national and ideological) prejudices. Our only means of avoiding parochialism and our only hope of building substantively important and interesting theories is to encourage the growth of a thriving comparative research.

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5. Walter G. Runciman, *Sociology in Its Place* (Cambridge: Cambridge University Press, 1970), pp. 10, 34. The best examples of such an eclectic approach are the writings of Max Weber.

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13. Concerning problems of school integration see James S. Coleman, Ernest Q. Campbell, and Carol J. Hobson, *Equality of Educational Opportunity*, U.S. National Center for Educational Statistics (Washington, D.C.: U.S. Government Printing Office, 1966). The rationale for the emphasis on early childhood education is based on a large number of studies stressing the importance of early childhood experience for later development; see Edward Zigler and Irving L. Child, "Socialization," *The Handbook of Social Psychology*, Gardner Lindzey and Elliot Aronson, eds., 2nd ed., Vol. III (Reading, Mass.: Addison-Wesley, 1969), pp. 450-589.

14. Nelson, *op. cit.*

15. *Federal Statistics Report of the President's Commission*, Vol. I (Washington, D.C.: U.S. Government Printing Office, 1971), pp. 102-116.

16. Eleanor B. Sheldon and Wilbert E. Moore, *Indicators of Social Change: Concepts and Measurements* (New York: Russell Sage Foundation, 1968).

17. I am grateful to Professor Eric Weil for reminding me of this last point.

18. With the exception of statistical handbooks containing demographic, educational and economic information which are published by the various organs of the U.N. and the OECD.

19. Something like this has been suggested by Kathleen Archibald in "Alternative Orientations to Social Science Utilization," *Social Science Information*, Vol. IX, No. 9 (April 1970), pp. 7-34.

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20. It should be evident that by theories I mean sets of hypotheses about observed events and not dogmatic viewpoints which often pass as theories in social science.

21. See trend reports from *Current Sociology* (Paris: UNESCO), such as Dupeuz, "Electoral Behavior," 1954-1955; Bendix and Lipset, "Political Sociology," 1956; R. Hill, "Sociology of Marriage and Family Behavior 1945-1956," 1958; Srinivas, Daule, Shahani, Beteille, "Caste," 1959; Miller, "Comparative Social Mobility," 1960; and R. Hill, "Sociology of Family," 1963-1964.

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RONALD DWORKIN

Legal Research

My subject is jurisprudence, which lies between two disciplines—law and philosophy—and often falls between two stools. I shall report about research in law, though I shall make the point that legal research does not differ from academic work in moral and legal philosophy as much as one might think.

I start, however, with this problem. It is controversial what research lawyers should do, and even what standards should be used to judge whether their research is successful. I know that research scientists often have the problem of choosing which of two different types of research—pure biochemistry or medicine, for example—should receive more attention and funds. Some scientists also have the problem, from time to time, of trying to anticipate whether a particular piece of research will be put to harmful use by governments or industry, and if so whether the research should be carried out. These are difficult moral and political problems, but they do not affect the overall pattern of scientific research, nor are they as complex as the problems that academic law faces. For it is not plain, even in principle, what sort of research the legal profession as a whole should undertake. The history of academic law in any one country, and particularly in the United States, shows very different conceptions of legal research at different times. The two common law countries in which I have taught, Great Britain and the United States, even now have dramatically different versions of how academic lawyers should spend their time.

It is easy to overstate the differences, but on the whole the caricatures are sound. The American academic lawyer, at least at the larger and better known law schools, thinks of his research as a branch of research in the social sciences. He is a lawyer, he will say, but he "brings to bear" some learning in economics, or psychology, or philosophy. He wants to know not just what the law is in his field of specialty, but how it works, what effect it has on his community, and what it should be. The English academic lawyer observes a much stricter division of labor. He takes as his precept Oliver Wendell Holmes' famous statement that law students should concern themselves with what the courts will do and nothing else. His laboratory is the law library; he will study the connections between one set of doctrines and another, to see what order can be made within the rich but nevertheless insulated world of precedents and stat-

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utes. Both of these lawyers are engaged in research, but it is hard to believe that they belong to the same discipline.

This description of American and English academic styles is, as I said, a caricature. There are many American law teachers who would feel at home in Oxford, and some, at least, at Oxford who would feel at home at Yale. But there is no doubt that the leading styles of academic research in the two countries are very different, and since I am interested in the special problems posed by such distinctions, I shall ignore the various ways in which the caricature ought to be refined. I do not think that these differences in style are an accident of history, nor the result of a more or less arbitrary drawing of lines between one profession and another, or one branch and another of the university. On the contrary, I think that the fact that legal research can take such different forms itself raises important questions.

I shall break the argument of this paper into two parts. In the first I shall try to show that very different conceptions of *legislation* underlie the differences between the two academic styles I have distinguished. In the second I shall argue that what I have called the American style of legal research demands, if it is to be successful, a radically different kind of institutional structure than now supports it. I shall, however, stop using the designations of English and American style because it will be less misleading to speak of a limited and a broad style instead.

I

The difference between the limited and broad styles I described is often said to be the difference between the study of the law as it is and of the law as it should be. If that were so, we might treat these as different and noncompeting studies, and consider the best institutional design for each. But that would misconceive the problem because the two styles represent, not merely different subjects different men have chosen to study, but different theories about the role that lawyers—that is, men with a particular kind of training and experience—should play in designing social institutions, and these theories in turn reflect different theories about the justification for these institutions. If we are to design devices to promote legal research, we must choose between the two models, though of course we can hedge our bet.

The cliché that some lawyers study the law that is while others study the law that should be is misleading in this way. It suggests that the limited style is concerned only with the mechanical job of repeating what the statutes and the judges have already said. But the doctrinal study of law which the limited style stresses comes to much more than that, for there is a distinct art of legal exposition that requires a developed and subtle technique. Suppose, for example, that an English lawyer wants to do research in the law of accidents. If he follows the limited style, he will take as his subject matter the variety of statutes and judicial decisions touching that subject in his jurisdiction, and in other countries or jurisdictions whose decisions influence the courts of his

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own. He treats these, however, as raw material only, because his job, not unlike that of the analytical philosopher, is to restate what has gone before so as to pick out the *principles* of decision, if any there are, that give unity to the collection. He seeks these principles for two reasons: as a matter of exposition, to give order to what may seem confused, and as a matter of doctrine, because the principles established in the statutes and cases are directions to judges in deciding further cases. Lord Atkin gave an excellent example of this art in his famous decision in *Donahue* v. *Stevenson*.[1] The earlier cases had developed a variety of rules about accidents—for example, the rule that the owner of an article "dangerous in itself" was subject to special liability for accidents it caused. Lord Atkin saw that all these rules could usefully be subsumed under a principle at once simpler and more intelligible, which became the principle of negligence: that anyone is liable for damage that could be avoided by care reasonable in the circumstances. I say "more intelligible" because this principle appeals directly to familiar assignments of moral responsibility in ordinary affairs, and so provides at once an explanation and a justification of the established rules.

I realize that there is some mystery in the idea I have used of principles "embodied" or "embedded" in or "underlying" the established rules, though no one ever knew they were there. I have tried to give a less mysterious account of this idea elsewhere;[2] the intuitive idea is that a general principle is embodied in a series of decisions if that principle is part of the most satisfactory coherent scheme of justification for the continued enforcement of these decisions. The idea is therefore at once backward and forward looking, and it is a crucial idea in all legal reasoning and research.

So it is wrong to suppose that what I called the limited style is either mechanical or easy. Neither is it in every sense insular, because it is a technique that can be used to great advantage in both comparative law and legal history. These studies, as well as ordinary doctrinal analysis, require the lawyer to bring a certain sort of order to a set of disparate materials, namely the order provided by the idea that a foreign or earlier legal system is built about principles that might be compared to, or seen as predecessors, of our own. Legal history and comparative law have for a very long time been considered subjects of legal research even in the limited style.

The limited and broad styles differ, not because the first is mechanical and the second subtle, but because the first is delimited and the second is not. We might compare the course of English research into accident law with contemporary American research in the same subject, as illustrated by the recent work of Calabresi, Blum, Kalven, Fletcher and others.[3] These lawyers have debated whether the established rules of accident law, which try to assign liability roughly on the basis of fault, like the principle of negligence I mentioned, makes sense when one looks at these rules the way an *economist* might. Is the principle of fault efficient in reducing the overall *costs* of accidents, however these might be described, or would a different system—for example, a tax on all motorists to provide a fund out of which all damages would be paid—be more efficient?

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Calabresi has argued that a non-fault system would be superior, while the others I mentioned, in different ways, contest that view. Their articles make very great use of economic theory and analysis, and plainly require economic training even to follow, let alone to produce.

I want now to invent a hypothetical, but I think representative, critique of this sort of legal research by a lawyer who follows the limited style. He would not, of course, for a moment suppose that this sort of economic analysis should not be done by someone somewhere. But he would have reservations whether it should be done by academic lawyers rather than economists, or made part of the legal rather than the economic curriculum. In his view the talents and training required for this sort of work are very different from those required for doctrinal analysis, so that if this work is done by lawyers both enterprises will suffer. The economic analysis will suffer because, at least ordinarily, neither the academic lawyer nor his students will be sufficiently adept at economic theory to make serious advances. The law will suffer because this blurring of the line between the two disciplines may corrupt the distinct and independent style that the lawyer needs for legal research. The lawyer and the economist, in this view, speak to different constituents with different problems. The lawyer speaks to his clients, and to a profession whose job it is to bring established doctrine forward so that individuals and firms may organize their affairs about it, and so that courts may interpret it intelligently in adjudication. The economist speaks, as it were, to the legislator, because it is his job, and not the lawyer's or the courts', to revise established doctrine in radical ways recommended by economic theory. Of course, the limited lawyer does not insist on strict rules. He recognizes that some lawyer may also be a brilliant economist; if he is it is all to the good that he do economic research in whichever part of the university gives him shelter. But the two academic careers, in this view, are distinct, and academic institutions like law schools should not be designed to merge them.

There are, I think, several mistakes of judgment in this hypothetical critique, but I want to pick out only two. It is a mistake, in the first place, to suppose that the traditional work of doctrinal analysis I described can be carried out properly without appeal to theory and research in the social sciences, like economics and psychology. I said that the essence of legal analysis is the discovery of principles that justify and bring order to the established rules and in this sense underlie them. Traditional legal analysis finds principles that, like Lord Atkin's, justify and unify because they appeal to familiar moral conceptions. But it may well be that a coherent justification of a complex field of law, like accident law, can be justified only by a complex set of principles some of which state moral conceptions, but others of which state economic or psychological policies which can only be identified or criticized through training in these fields. In fact, the work of Kalven and Blum, among others, tries to do just this for the existing framework of accident law; they try to show that the intricacies of the present fault system can be understood only by supposing principles of economics as well as morality to count in their justification. If this is so, then the critique invented fails on its own suppositions, for then the problems of counseling and

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adjudication to which the limited style is addressed require training in the disciplines it seeks to exclude.

The second mistake is the complement of the first. It is wrong to suppose that the traditional techniques of doctrinal analysis which the limited style emphasizes are irrelevant to the problem of legislation—that is, to the problem of designing new or improved legal structures. I shall pass the obvious though important point, that the design of new laws must take account of problems of publicity, compliance and enforcement, and possibly the design of new legal institutions like administrative agencies, about which the lawyer alone is expert. I pass that point, because the proponent of the limited style might hold that these procedural considerations are distinct enough from the substantive issues so that they might be made the subject of legal study even on the limited model. The mistake I have in mind is more fundamental, because it is an important mistake in political philosophy.

The limited style assumes that the sort of research appropriate to legislation is only what we might call instrumental research—that is, research into the techniques of driving society forward towards some goal already established and that this sort of research can be assigned without residue to one or more of the distinct social and physical sciences. It assumes that non-instrumental issues, like the question of which goals society should seek to realize, are not the subject of any professional skill. It assumes, to continue the example of accident law, that the question of how to reduce accident costs or other acknowledged social evils is a technical question for the economist, while the question of what shall count as an accident cost or a social evil is a matter for collective choice through the political process—a matter of "values" and hence not a matter of research for anyone, except perhaps for moral philosophers or theologians, but certainly not for lawyers.

I recognize that it is controversial whether these assumptions are indeed mistakes; many people believe that it is the beginning of wisdom in the social sciences to understand that research ends where values begin. I shall try to illustrate my point by changing my example from accident law to race law, a field of law where the issues of "value" are more notorious. The race issue in the United States is now dominated by the question of school busing. What programs should be adopted to cure or at least to reduce the effects of decades of illegal or unjustifiable racial segregation in both northern and southern schools? Should these programs include "busing" of children of one race into neighborhoods where the population is dominated by the other? Plainly these questions state issues for educational psychology, educational sociology and indeed economics. Plainly they also raise issues of principle. Suppose—and it is fair to suppose this—that the overall level of education will suffer from busing. We must then ask whether it is right that educational policy should recognize goals beyond the quality of education—for example to improve social equality. We must ask whether citizens are entitled to the best education that the money available can provide, at least overall, or whether, on the contrary, they are entitled to the most equal education that the money available can provide.

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Should we say that the white citizens share some sort of blame for the deprived standing of black school children, or at least that they have been in some sense, to use the lawyers' phrase "unjustly enriched" by the earlier unequal allocation of resources? Shall we say, therefore, that it is now fair comparatively to disadvantage white children by busing if that will help the blacks? The questions multiply in all directions, but I have listed enough to show that they raise issues of "value" as well as strategy.

I suggest that these issues must be tackled by deploying principles of equality and fairness and a theory of social and individual rights sufficiently general to provide the support of consistency but sufficiently concrete to guide legislation. I also suggest that lawyers are particularly well suited to join, at least, in this enterprise, that they ought to do so in an institutional way encouraged by the design of law schools, and that in doing so they will need to draw on training in philosophy and the social sciences, as well as on the traditional doctrinal skills that the limited style demands. I cannot argue that elaborate brief fully, but I urge the following points in its support.

I shall start with the last point. It is, I think, a necessary condition of any theory of legislation that the principles and policies it deploys are not contradicted, but instead are affirmed, by other social programs that the same community sponsors. I know the old saw about consistency, but that old saw is a fraud designed by a feeble mind for its own protection. Lawyers deal professionally with the full range of social decisions that depend upon principle; lawyers can ask whether a theory of punishment or unjust enrichment relied upon to justify busing is consistent with the principles assumed by the law of property generally, or the law of taxation, or indeed the criminal law, and they can imagine the consequences of settling on a new theory across the sweep of these fields of law. They can ask whether a right to quality education has been assumed by educational institutions as they stand, or by the distribution of local taxing powers, and if not whether it would be right to compass the changes that recognizing such a right would demand. They can ask whether the theory of equality proposed by the advocates of busing is one that can be endorsed generally—whether it goes beyond the affirmations of the theory of equality needed to explain the constitutional decisions of the Supreme Court, for example, or the theory of equality that must be assumed in order to support whatever else the country has demanded of its citizens at the cost of efficiency in the name of equality.

In all this (and I only suggest the beginning) the lawyer draws on the idea of principles embedded in institutions that I described earlier, though perhaps with more imagination because he must try to justify the changes he wants or is willing to make as well as what stands already in the books. But the same instinct for principle and generalization that the common lawyer needs is at work here. Of course the talent I now describe is that of the practical philosopher as well as of the lawyer.

But what about the social sciences? If the lawyer must be a philosopher, why must he be an economist, or a social psychologist? If he must find principles

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of fairness, why must he study to find principles of sociological theory as well? The short answer, which I am afraid masks a very complex argument, is this. The lawyer, as practical philosopher, must be sensitive to the fact that the social sciences may provide concepts that he must understand and use. This may, in part, be simply a question of vocabulary. If utilitarianism is in the end the right standard for the design of accident law, the precepts of utility can nevertheless best be stated, in a form useful to the economist who must work out the utility functions, in economic rather than philosophical or even ordinary language. If principles of equality do require concern for equal respect as well as equal pay, these principles may be put in a more effective form by using the concepts of individual and social psychology. But the point goes beyond vocabulary to imagination. The concepts of social science offer new principles, not simply new ways of describing old ones. The concept of personality, in the form developed, for example, by Goffman, describes a different set of expectations and concerns to be protected than any concept readily available in ordinary language. The economist's concept of efficiency, which is different from the philosopher's principle of utility, offers a new and useful idea of combining profit with fairness.

II

You may or may not be persuaded that what I called the broader model of legal research is superior to the more limited. But you may well be persuaded that it is impossible to realize the broader model in any useful form. I have indeed described an academic superman, learned not only in law but in philosophy and expert in all the social sciences as well. But supermen do not exist, and you may well think that when one is born he will not choose law for a career. So the problem for institutional design is this. Can we come close enough to achieving the broader model on a wide scale to justify trying? Or will we simply ruin a good discipline—academic law in the limited style—by trying to change it into something else that cannot be achieved?

It is true, as I suggested, that the more prominent American law schools have each tried, with varying degrees of commitment, to extend themselves towards the broader style. An enormous amount of money has been advanced by foundations to law schools for this purpose, and we have had a great variety of institutes of "law and social science," either independent or attached to law schools. These have produced some outstanding work, but it is not plain that the money spent was well spent, and it is by no means clear that the integration of law and other disciplines in the manner called for by the broad style will ever succeed in the way that its champions have hoped.

I do not propose here to evaluate any particular programs. Instead I want to raise the suggestion that a more radical change in the nature of legal education may be required. I shall try to do this by considering the fundamental question in an abstract way. How might we provide the union of skills, learning

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and talent that the broader style of academic legal research requires? We might distinquish three different techniques.

1. *State-of-the-art learning.* When American academic lawyers, in the twenties and thirties, began to realize that they could not teach law effectively without a knowledge of social science, they recommended to themselves and their students a course of education through which lawyers might learn what was available from other disciplines. If a lawyer wished to study commercial law or monopolies law, he would inform himself about the state of the art in the branch of economics closest to these subjects, and if he was concerned with the law of punishment he would read the latest treatises in psychoanalysis in the same way. But this sort of *ad hoc* dipping into other disciplines has proven unsatisfactory for obvious reasons. The research of a discipline is directed to the problems it identifies, and if no one has asked an economist to study the special economic problems posed by different classes of stock, it is unlikely that there will be very much in the literature of help to the lawyer who is attempting intelligently to draft a new corporation code.

2. *Collaboration.* The progressive idea therefore developed that lawyers and social scientists should work together, with the aim, chiefly, that the lawyer would guide the social scientist by setting problems in economics or sociology specially adapted to the lawyer's needs. (I say "chiefly" because some social scientists who became involved had a different idea; they sometimes thought that the lawyers were the junior partners, who were provided to answer legal questions that the social scientist might encounter.) In any event, various designs evolved. Many law schools hired, as regular members of the faculty, members of other disciplines who had no particular training in law, and these non-lawyers were available both to teach students and to work with their legal colleagues on joint projects. In some cases, the program was expanded to include small institutes attached to law schools. These programs have obvious advantages over the state-of-the-art learning technique. They work best when the lawyer is able to frame his question in such a way as to provide an experimental program for the social scientist to begin on. It might be important for the lawyer to know, for example, what the deterrent effect has been of a particular increase in the punishment for a particular crime, or how the maternal mortality rate has been affected by a change in the abortion law. In this sort of case, he needs the research skills and strategies, as well as the mathematical techniques, of the social scientist, and he is able to put to that scientist a sufficiently well formed question to allow him to proceed. But collaboration of this sort is likely to be much less successful in dealing with the major problems of principle I described in the last section. If we aim at improving the lawyer's imagination by offering him a new set of concepts through which he might develop genuinely new principles with which to state legislative concerns and goals, then we must work towards an ideal that goes beyond collaboration. We must try to make the lawyer sufficiently at home in other relevant disciplines so that his concepts might provoke new ideas in him. In fact, the best products of the broader sort of research in law so far have developed through the work of

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the occasional men, like those I mentioned, whose achievements can be described as achievements in two disciplines not one.

3. *Ambidexterity.* So we are back with superman—or a reasonable approximation. How can we encourage a sufficient number of scholars with legal training to become not merely familiar with some other discipline, but familiar enough to develop theory within it? Such a lawyer must have not only undergraduate but graduate training in another discipline, and as law school is itself a graduate program in America and an exclusive undergraduate program in England, it is unrealistic to expect that more than a very few people will complete what are in effect two distinct graduate programs. Of course some academic lawyers have done so—the faculties of the major American law schools include several lawyers who have Ph.D.'s in other fields—but as the costs of education increase it is doubtful that the number able to do this will also increase.

If we are serious about extending legal research on the broader model we must arrange legal education to encourage it. But the minimal change required seems to me a radical change. Law has grown up as an academic discipline, both in America and other countries, principally for the training of practicing lawyers. American law schools were designed for this purpose, and in their orientation they looked more outward to the professional community than inward toward the rest of the university. Academic lawyers received the same training as professional lawyers, taking the same programs in the same schools, and only rarely enjoying special graduate training that the practicing lawyer did not receive. This made some sense when academic research was envisioned on the limited model, because the training in doctrinal analysis the academic lawyer needed was much the same training that his practicing brother had had; indeed, it used to be common for academic lawyers to spend several years in active practice before returning to the university, on the ground that this was necessary to "complete" their legal education. But academic research on the broader model plainly requires a different kind of training from the training of a practicing lawyer, and it is time to inquire whether legal education must not recognize, in its instituions and practices, that difference. At a minimum the major law schools should consider instituting two different programs, perhaps a shorter program than they now offer for men who wish to begin practice and a longer program, much more integrated with other departments in the university, for those who want a career in academic law.

But even that sort of change, though it would be remarkable in legal education, would be unlikely to provide much of the ambidexterity in two disciplines that seems called for. No doubt the equivalent of some graduate training in, for example, economics, could be made part of a four year graduate law school program for future academic lawyers. But if we expect from a man innovative work in economic theory, that cannot be relied upon to be enough. It may be that we have stuck too closely to one model integrating law and other disciplines. It may be that we should not think only of supplementing the lawyer's education with a bit of social science, but also of adding legal work to the ordinary training of the social scientist.

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That might be done, to some extent, by arranging a program within law schools for graduate students and postgraduates in the social sciences. Some American law schools have started pilot programs of that sort. The Yale program for sociologists, and a program now planned for philosophers, follows that model. But these programs are designed in the nature of the case for a very small percentage of those who are doing graduate work in social sciences, and indeed for a self-selected group who have decided in an early part of their career that legal theory will be a major interest. We ought to aim at a program that will bring a sense of legal theory—a sense of practical philosphy—to the education of social scientists generally.

This idea is radical because it reverses the usual supposition that legal training builds on a platform of a more cultural or scientific education. But it may be more realistic to expect a significant advance in legal research from an economist or a philosopher who has once studied law, than from a trained lawyer who has studied philosophy or economics. If, as I argued, legal theory informs the work of these other disciplines in a very general way, and is not simply a specialized input of information for particular specialists, then we must provide mechanisms for making legal experience available to the standard and not only to the exceptional career in social science. This cannot be done except by a more thorough integration of jurisprudence into the university than seems possible if law is left the exclusive domain of the distinct graduate professional law schools.

III

We must therefore explore ways of bringing jurisprudence into the graduate and perhaps even the undergraduate curricula of the social sciences. It is already there, of course, in one sense. Most universities provide some courses in which law is studied outside law schools. Political science departments commonly offer some survey or history of aspects of constitutional law, for example, and within the last five or ten years courses in the philosophy of law have become much more common. But these courses divide legal materials in accordance with prior divisions between disciplines, and they conspicuously do not provide training in the integrated analysis of the principles and policies underlying law.

If we wish to train non-lawyers in the broad style of legal research, we must design programs that are aggressively and systematically inter-disciplinary, and that are organized around problems rather than contributory departments of a university. We must have courses in jurisprudence, that is, rather than courses which set out a sociologist's or an economist's or a philosopher's concerns with law. I shall try to describe a model program which begins with a few experimental graduate seminars, and then builds towards a wider section or division of jurisprudence as a distinct organization within the university. The program is launched with three or four seminars of the following description. Each is led by a team which should include a lawyer, a philosopher, and at least one representative of the social or natural sciences. (I recognize that this staffing is expensive, even luxurious, but at least during the first few years some of those

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faculty participating in the seminars should be willing to do so in addition to ordinary course requirements.) The seminars should be limited to graduate students selected so as to give the disciplines represented in the staffing appropriate representation among the students. Law students should be admitted but should form only one segment and not dominate in numbers. Each seminar should attempt to work out and criticize the moral, economic, social and psychological principles or hypotheses on which some complex legal doctrine or set of doctrines might be justified. I offer some examples of such seminars.

1. *Accident law,* taught by a lawyer in the field of torts, a moral philosopher, and an economist. I offer this example to continue the example offered earlier in the paper; the literature I described earlier itself shows the need for research that is genuinely interdisciplinary. It has been marked by legal and economic but not philosophical sophistication, and consequently little work has been done on a fundamental issue that accident law ought to illuminate, which is the issue of whether justice in the law of torts is distinct from efficiency, and, if it is, how each of these two goals figures in the definition of the other.

2. *Constitutional law,* taught by a lawyer, a philosopher and a political scientist. I just said that most universities offer surveys of constitutional law for undergraduates and graduates, generally through the political science though sometimes through the history department. But these courses typically neglect the fact that constitutional adjudication, particularly in recent years, has drawn more upon moral and political theory than upon empirical political science.[4] At the same time, while there has been a renaissance of interest in political philosophy, particularly since the publication of John Rawls' important book, *A Theory of Justice,* political philosophers have not extended their concern to constitutional legal issues, and both constitutional law and political philosophy thereby suffer.

3. *Criminal responsibility,* taught by a criminal lawyer, a philosopher working in the philosophy of action, and a psychologist. We have had, for some years, two sorts of interdisciplinary work in this field: work connecting the law of responsibility with mental philosophy, as illustrated by the collection of essays[5] recently published by H. L. A. Hart of Oxford, and work connecting that law with psychology, as demonstrated in the books and teaching materials of Abraham and Joseph Goldstein, and Jay Katz, at Yale. This work must now be brought together, particularly in the study of the enormously complex conceptual and moral issues relating to criminal insanity.

4. *Legal institutions,* taught by a lawyer whose field is legal procedure, a philosopher of law, and an institutional economist. For many years the materials on legal process[6] prepared by Henry Hart and Albert Sachs of the Harvard Law Schools have been the basis for important and popular law school courses. If they were revised and adapted for graduate use, and subjected to the critique of other disciplines, they would make an admirable training exercise for social scientists anxious to understand how the broad legal style can be directed not only to substance but to issues of fairness and efficiency raised by the legal process itself.

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These are obvious examples of the sort of seminar I have in mind; experience and imagination could produce a much better list. There are, no doubt, a variety of ways in which such seminars might proceed. The simplest would be this. The lawyer in the seminar team would present, in the early meetings, a survey of the state of the law in the field on which the seminar would concentrate, pointing out, to the extent this exists, the conventional rationale for the leading doctrines, and indicating the principal areas of doctrinal controversy. Other members of the team might then offer critiques of the conventional rationale, and then offer, from the standpoint of their disciplines, a set of principles that would either form a more adequate justification of the main lines of the existing law together with suggestions for modifications in the light of that more adequate justification, or serve as the basis for a more radical reconstruction. Each of these critiques would overlay and attempt to absorb what was accepted from the others, so that representatives of each discipline would acquire not only sophistication in the other disciplines, but some facility in integrating their assumptions and methods into a more fluent theory which, if the seminar were successful, would not belong distinctly to any of the disciplines initially represented.

I have not much to say about the administration or costs of such a program. Presumably it would be under the direction of a special committee which would enjoy a budget and might purchase the time of faculty from the law school and the departments of the university. The program, in a large university, might expand to cover a much larger number of seminars, which might then be arranged so that some would be natural precursors of others in a graduate program. If they were successful, the literature they produced might then serve as the basis for a pilot undergraduate class along similar lines.

The university which launched such a program would hope for a variety of gains. The experience of the faculty, working together on problems rather than within disciplines, should prompt research of a distinct sort, both theoretically imaginative and directly useful to a professional. The long term gains, however, would be larger from those graduate students, never destined to be lawyers or even to work in a distinctly legal branch of their discipline, who would nevertheless work in their professional careers with a better sense of the legal questions they might help to answer and to ask.

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HARRY G. JOHNSON

National Styles in Economic Research: The United States, The United Kingdom, Canada, and Various European Countries

ANY ATTEMPT to discuss national styles in economic research, and the institutional arrangements that influence them, has to start with recognition of the fact that in the post World War II period, and especially in the sub-period beginning somewhere in the early or middle 1950's and running up until two or three years ago, economics has been undergoing what may be called a "research revolution." On the intellectual or purely scientific plane, the 1930's witnessed the production of a variety of important new ideas. The most apparently revolutionary idea was Keynes' *General Theory of Employment, Interest and Money*—though from a policy point of view the theory of monopolistic competition has also had a significant impact. But with specific reference to research, the most important ideas, from a longer-run point of view, were the introduction of mathematically-based general equilibrium theory derived from the Continental theoretical tradition, the acceptance of mathematics as a more powerful analytical tool than literary logic and commentary, and the transmogrification of economic statistics into econometrics—the use of statistical methods to measure and test relationships specified by theory.

These ideas were barely launched before the interruption of scientific economic activity by the Second World War, which diverted a large proportion of English-language economists—and an even larger proportion of those who otherwise would have become graduate students—into work in the government economic service or into the armed forces. After the war, many of the prewar generation of economists returned to academic life, but typically with a commitment to positions they had adopted in the 1930's and without the drive to resume the hard intellectual exercise that characterized them earlier. On the other hand, thanks in large part to generous veterans' benefits supplied by governments, the number of graduate students expanded greatly; and those students were quick to absorb the most readily learnable aspects of the discipline and the aspects most likely to give them both respect from their teachers and demonstrable superiority over them, namely the new techniques of mathematical analysis and econometrics, as contrasted with the social wisdom and philosophizing that characterized the older style of "political economy." Hence economics shifted from an orientation towards "political economy" to an orientation towards "eco-

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nomic science"; and the essence of science is research, the application (with more or less ingenuity) of known or knowable techniques to the solution of suitable problems (which may or may not be of general social interest, but usually are not because problems of general social interest are usually very untidy).

The relevant point about the "research revolution," in the context of the present discussion, is that its focal point and indeed its revolutionary frontier were located in the United States. For this there were many reasons. One was the intellectual tiredness, the sense of themselves as anachronisms, and the inferiority complexes of both continental Europe and the United Kingdom after six years of an all-out war which was finally won by the resources of the United States and Russia, together with their lack of resources and intellectual capacity to mount the kind of graduate training program required to equip students for the research revolution—and the repugnance of such programs to the historical tradition of the lonely scholar working on his own and deriving his satisfaction as much from the work itself as from sharing its results with others. Another reason was the historical dependence of ex-colonial countries like Canada and Australia on the mother country to provide ideas, teachers, and graduate instruction for their own students; they were incapable of picking up the ball of the research revolution themselves but their teachers and students saw clearly that the United States was the place to get graduate training and began to go there rather than to the United Kingdom or Continental Europe. (The European students, especially those from the United Kingdom and northern Europe, got the same message; those from France and Italy were inhibited from studying in the United States by the difficulty of fitting themselves back into their own university systems—with the result that France and Italy are of virtually zero significance on the contemporary economic scene).

The most important reason, however, lay in the characteristics of the United States itself. First, in contrast to Europe, and to a lesser extent Canada and similar countries, where a professor is a member of the national elite and is accordingly judged by his behavior in relation to what the elite think he should be doing and saying, so that he has to be in some sense a "rounded" member of society, in the United States—except for brief periods of political esteem for what the academic can do to solve society's problems—the academic world is a fairly self-contained world, within which entry and promotion are fairly strictly governed by academic standards. Hence students attempt to acquire, and aspiring universities try to provide, instruction in techniques of analysis and research that will give them an edge over their rivals and the promise of future publication performance. Second, in contrast to the European tradition of doing graduate work on one's own under an eminent professor or supervisor, the United States has a long tradition of explicit and organized programs of graduate instruction, a tradition in which the program adapts itself sooner or later to the incorporation and transmission of new developments in the field. Third, and related, the United States has the resources to support such programs by the offer of handsome fellowships to students of high quality, and competition for good students among university graduate departments extends this support to for-

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eign as well as domestic students. Fourth, the interest of American society in knowledge has always been pragmatic rather than philosophical, so that an orientation towards techniques and research comes more naturally to the American than to the European scholar. Finally, the U.S. concern for the promotion of science and scientific research that followed in the wake of Russia's success in launching the Sputnik in 1957 extended to embrace economics as a science, thereby creating an inflated demand for "economic scientists," i.e. those who at the junior level would demonstrate satisfactory training in mathematics and econometrics and at a senior level could prove research performance by published output of ceremonially adequate scientific papers.

The results of this outburst of technique-and-research-oriented training and activity in the United States have been that the profession of academic economics in most countries, including the United States, is divided rather uncomfortably into three groups: those who have been to a top-notch graduate school in the United States, or a school there or elsewhere good enough for them to have acquired the research techniques and orientation, and who have stuck with them in a research-and-publication-oriented career, wherever they may be teaching; those who have attended a good graduate school to acquire the qualifications necessary for respectable academic employment, but thereafter have receded into non-research interests and activities, regarding a bare minimum of research as a necessary cost of academic reputability; and those who have never gone through the mill of a top-notch graduate school, though they may have attended one as a short-term student or a senior visitor.

The first group constitutes the elite of the profession—those who communicate with each other by exchanging papers before publication and by attending international conferences, rather than by reading each other's work with a long lag in journals and a still longer lag in books. Their style is an international style, though primarily American in origin and outlook, and frequently they have to maintain it in the face of hostility and resentment from their departmental colleagues and of inadequate institutional support. One reflection of this problem is the strong desire on the part of such people to establish research institutes with financial support from outside the university, in order to free themselves from the restrictions imposed on their research ambitions by the departments and universities within which they work. Another is their anxiety to take as much leave of absence as possible from their main employer university to spend visiting, or teaching if necessary, in one of the livelier American (or even British) graduate centers. Still another is their eagerness to obtain short or long term visitors from the international world who share their style of scholarship, through the offer of visiting lectureships and professorships and the holding of conferences, the published proceedings of which will serve as validation of their belief that they belong in the large cosmopolitan world of economic scholarship and research.

It is the other two groups that constitute the repository of differences in national styles. And these differences reflect differences in three major factors: national culture; university tenure and promotion systems; and outside funding

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for research. With respect to national culture, it would perhaps be more accurate to subdivide that factor into differences in national culture, and differences in the way in which the economist, either as such or as an academic generally, is involved in that culture. One could discuss the factors separately, using different nations as illustrations, but it is more convenient to compare nations in terms of the influence of the factors. (The reader is warned that what follows consists of condensed personal impressions, which may well be considered offensive or inaccurate by other people).

In the United States, the economics profession has shared the general freedom of the public to disagree with its government—except in wartime, when all the population is expected to close ranks in defense of its peacetime freedom. In this respect it has had the advantage that its relation to government has been that of a hired independent expert; and since the profession is large, there are always competent economists available who will work for a government of any political complexion, and who will neither suffer opprobrium from their fellow economists nor enjoy undue professional advancement for so doing (in this respect, economists are probably a tougher intellectual lot than, say, atomic physicists). In other words, the national culture values economists primarily as experts, not as potential or actual rulers.

Perhaps as a result of this respect and freedom, the most impressive characteristic of the economics profession in the United States is its consistently high quality across the country. Wherever one goes to visit a university, one finds people who have been very competently trained, who maintain a keen interest in the subject and its development, and who have a serious research interest of some kind. In part this may be an optical illusion: the rapid expansion of American universitites in the past decade has meant that most departments consist predominantly of young faculty members ("young" means under forty), and the few seniors, unless they are really competent, have been squeezed out of real participation in the department and can be kept decently hidden from sight. Discounting that fact—which in any case applies to other countries as well—the explanation for this excellence consists of a number of factors. One is the rather isolated role of the academic in the American society, already mentioned, which tends to concentrate his attention on his subject. Another is the pragmatic nature of American society, which in this connection means that a university teacher has to take his stand, not on the greatness of his personal thoughts about and reactions to life, but on how good he is in his subject and how well he can teach it. A third is the ability of American universities to insist on the Ph.D. as a qualification for academic tenure, which means that most people teach in a university of lower quality than the one from which they received their Ph.D., are conscious of that fact, and are anxious to prove to themselves and others that individually they are better than one would expect from their institutional affiliation. A fourth is the stringency of American standards for promotion and tenure, which typically require a man to continue to demonstrate competence by research and publication well into his forties. Allied with this is a fifth, the mobility of American academic life, which means

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that a man who demonstrates research competence may at any time receive an offer of better pay and higher rank from a university of higher (or equal or not much worse) quality than his present one, which will enable him to bargain for a better deal from his own university. In other words, the academic in America is being continuously evaluated, by his own or other universities, until a very late stage of his academic life; and this provides strong incentives for him to keep up with his subject and to engage actively in research. An important point here is that in contrast to the British and other university systems, pay and title are based largely (though not always) on performance and not on age. A sixth factor is the multiplicity and diversity of the American governmental structure, and the geographical dispersion of the country. This means that if an individual is looking for the opportunity to do sponsored research of an academically respectable kind, and is prepared to turn his mind to other people's definitions of research problems, he can usually find research support from some branch of the federal or his state government, or from some local industry or national industry with a local branch, or from some foundation.

This implies, of course, that research in the United States tends to be "outer-directed" rather than "inner-directed." This is probably not too serious a defect, given a plethora of competently-trained people who are obliged to do research of some sort but lack the originality to devise their own problems, and the scarcity of original ideas anyway. However, the general tendency is for research to be directed to small problems rather than large ones. To correct this tendency, and to steer research towards problems of more general and immediate national interest, the United States has developed the technique of granting government and foundation money for research. This system, however, has its own defects. It encourages people to venture beyond their intellectual depths in order to get research money (also to organize big research teams that are not necessary to the purpose in hand and are hard to administer) and to produce research results that are technically competent but of little value in relation to solving the original problem. Also, the foundations, because their administrative structure calls for regular turnovers of staff each new wave of which seeks to monumentalize itself by initiating something new and imaginative, exercise a disturbing influence on research by encouraging economists to embark on research endeavors that have no long-run future, incidentally leaving universities stranded with staff whose research interests are at variance with the university's central teaching responsibility.

Mention should also be made of those foundations that conduct research themselves, either in-house or on a contract basis. The most well-known and influential of them, the National Bureau of Economic Research, which began as a powerful center for the quantitative study of business fluctuations, gradually became ossified and antiquated in its concepts and methods, and is struggling to re-establish itself under new management. For a long time, however, it exercised a strong influence on the direction and character of American economic research. In particular, it has supported the work of Milton Friedman, leader of the "monetarist counter-revolution" (against the "Keynesian Revolution").

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More uniquely characteristic of the American scene, however, are a number of foundations devoted to the study of policy problems—the outstanding one being the Brookings Institution, which has played a very active entrepreneurial role in the funding of research in recent years. The studies supported by these foundations provide an ongoing independent research-and-theory based evaluation of policy problems and alternatives, something that governments do not have time for, and that in other countries non-governmental agencies and academics lack the resources and the incentives to undertake.

For several reasons, the situation in the United Kingdom is very different. For one thing, the United Kingdom, with which I am most familiar, has been bitterly scarred by its historical experience. The period before World War I gave its economists the impression of an incredibly affluent society, in which context all sorts of free thought were possible and only the just distribution of the affluent income constituted a real economic problem. But two world wars, and the associated capital losses and recognition of superior American affluence, have closed in the intellectual perspectives of British economists. The problem is no longer to determine what is true about the economic side of society, but how to use economics to defend the British economy against the onslaught of adversity and how to work the miracle of restoring Britain to its former position of economic dominance (or at least respectability) in the world. Thus economics is converted from a scientific subject into a species of political necromancy.

Furthermore, as the affluence of the British (or European) economist declined, so did his political independence. There was a time when an economist was also a propertied gentleman; if asked for advice, he would give it—frequently he published it before being asked—but he was an independent citizen who did not identify himself with the country's rulers though he thought of himself as an equal. That situation has changed, again mostly as a result of two world wars and the emergence of the United States and Russia (and more recently China) as dominant world powers. The result has been that the national society has closed in on the economist, and he has lost his role as an independent citizen distinguished by his scholarly knowledge. Instead, he has tended to become an intelligent parrot, concerned either with maintaining that his country is still as great as it used to be—or could be if only it followed his advice—or with asserting that if it would only follow some policy adopted by the dominant countries it could restore its greatness.

This politicization of the economist is not unrelated to the academic situation in the United Kingdom, where a B.A. plus some research time at a "good" university (rather than the Ph.D.) is all that is required for an academic post, so that there is no formal graduate training in research methods and little real emphasis on research other than as a minimal requirement for promotion and tenure. In addition, salary scales are more or less constant across the country and salaries increase with age regardless of performance, apart from the points of major review: for tenure, for the lecturer efficiency bar, and for the possibility of elevation in rank to Senior Lecturer, Reader, and Professor. Ser-

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vice to a department and university rivals, and sometimes takes priority over, research performance as a criterion for promotion. Moreover, there is a tradition set by the Oxford tutorial and Cambridge supervision systems for undergraduates, that a teacher proves his academic quality by his ability to find fault with the published work of others, a tradition which inhibits the average academic from sticking his neck out by publication of research on any but the most innocuous and dull subjects, while at the same time giving free rein to charlatans who do not care what is said about them so long as their name is spelled correctly and they get a reputation for originality. The Oxbridge tradition, which involves training economists primarily for the civil service or politics, and only secondarily for academic careers, has also perpetuated a polarization of the profession into left wing and right wing which originated in the deep political tensions that rocked the country in the interwar period. This has been a particularly serious impediment to scientific research and progress because both Oxford and Cambridge have been held by the left wing since the war, and, through their eminence over other universities in British cultural and academic life, they set the public image of economics as a political subject and maintain it by the practice of applying political criteria to the assessment of scientific ability and achievement. One result has been to steer aspiring young economists out of main-stream economics into mathematical economic theory and theoretical econometrics, fields in which British economics has continued to maintain a reputation for international quality.

The general tendency in Britain towards desultory and dull work on unimportant problems is reinforced by the nature of research support, and more generally by the social position of the academic economist in relation to government. The imperial history of the country, and the two world wars through which it has passed, have created an ethos in which it is accepted on both sides that the academic is generally inferior both socially and intellectually to the civil servant—an acceptance reflected in the relative pay scales of the two. His function is to do humdrum research of mundane usefulness to government, applying whatever part of existing knowledge he has encountered and is capable of apprehending. The function even of the better academics is either to provide sophisticated rationalizations for government policy decisions or to provide new and challenging ideas on policy that government (or its opposition) might be able to use sometime. This function involves no research at all, only cleverness in making forensic use of other people's research for political purposes. In accordance with this socio-political structure, research money is available in austere amounts for applied research projects deemed socially useful; in much more generous amounts for applied research projects demonstrably useful to governments such as the construction of large-scale forecasting models of the British economy, represented by those of the National Institute for Economic and Social Research (which is essentially an indirect branch of government and produces virtually no basic economic research worthy of the name), and of the London Business School (the large University of Southampton model is designed for relationship-testing rather than forecasting); and in

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still more generous amounts for academic economists prepared to take leave from their universities and accept much higher-paid temporary employment in government in order to write policy-oriented papers that rarely if ever get published, except when necessary to refute academic criticisms of government policy decisions. It is no wonder that Britain has completely lost the reputation it established in the 1930's as the world center for the serious scientific study of economics, and that its only hope of regaining its repute lies in the small group of its economists who belong to the American-based world community of highly-qualified and research-oriented scholars.

In Canada, the colonial tradition still prevails: "Them as can, do; them as can't do, teach." The Canadian academic has an uneasy place in his national culture: his society believes that he is overpaid for the mere possession of knowledge and the imparting of it to the young. He has two alternatives. The majority choose to acquire still more learning without using it to raise any new questions about Canadian society, though they may legitimately use it to refute the wilder public answers to old questions, provided they do so decorously and without having any real impact on public opinion. A minority choose to abandon all academic standards and to use academic status as a platform from which to encourage the wildest of public emotions, especially in support of a form of nationalism which would guarantee its academic exponents academic promotion without academic delivery.

These tendencies are encouraged by the slackness of the university system itself. Most of the better young Canadian economists, by now, have been educated at American graduate schools; but of these, a large number regard acquiring a Ph.D. as an initiation rite into adulthood of an extremely painful kind that does not have to be repeated, while those who have acquired the scientific research orientation find that it has little pay-off once they return to Canada. For Canadian universities have slack tenure and promotion practices, in which age and service to the university count for more than research performance, and there is little competition among universities for staff of superior qualifications. Research does, however, get done, as a result of the fairly generous funds available, but the research is not necessarily of a kind scientifically useful from a long run point of view. There are two main sources of funding. One is government, which finances research either directly for a summer vacation or for one or two years' leave or indirectly through successive Royal Commissions. This kind of research involves the application of existing techniques to applied policy problems and does little to advance basic knowledge in the science as a whole; concern has in fact been expressed in Canada about this aspect of the dominance of government funding of economic research. The other source is the Canada Council, which has ample funds without strings tying them to applied research, but which tends to grant applications which are essentially for comfortable sabbatical leaves rather than for serious research of general interest. Since many Canadian economists at the present time are immigrants from other countries, or else have been involved in granting foreign aid, much of the money goes in reality on travel grants. (This, of course, is

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not the fault of the Canada Council, but rather the fault of the Canadian university system for not requiring its academics to deliver a sustained research performance, but instead allowing them to regard a year of idling at someone else's expense as a just reward for involvement in undergraduate teaching and university administration.)

In Europe, the north (Scandinavia, the Netherlands, and Germany), where competence levels are high and research is well understood, must be differentiated from the south (particularly France and Italy), where research is misidentified with literary facility and a capacity for grandiloquent rhetoric. France, for example, lives on dreams of grandeur and on rhetoric instead of scientific analysis from its economists. Big prizes go to those so-called economists who can snatch ideas from the English literature and translate them into French concepts and the French language; and due to the unreality of French culture in relation to the rest of the world, prizes are often won by snatching unscientific pseudo-philosophical ideas from the Anglo-Saxon world and translating them into even more pseudo-scientific French. In this respect, the most esteemed French economists are the most fraudulent economic scientists. Any hope that France has of restoring the scientific repute it had before it expelled Walras to an exiled existence in Switzerland lies in the undervalued economists who staff its technical schools, and in the young English-language-oriented economists who have risen to positions of power as a result of the university expansion precipitated by the events of May 1968.

All of the European countries, however, have some common convictions (vestiges of which also remain in the United Kingdom, especially in the provincial universities): that research is a necessary requirement for obtaining a professorship, but not a requirement for retaining a professorship once acquired; that the research in question has to be approved by the reigning professor, and support his long past accomplishments and current prejudices; and that a professor, by virtue of his past research, must be an expert over the whole field of economics, an expectation which leads him to believe that he is competent to be such—a belief supported, in turn, by the deference accorded him by his obligatorially deferential students and by his fellow-members of the elite. Academic research is not well funded; instead, it is subsidized heavily by the students who have to do it to make their own names and earn their professorships; and the subsidization extends to their professors, whose own research, such as it is, is largely extorted from the dependence of their students on their goodwill. It is thus not surprising that one encounters three types of Continental academic economists: the young man (probably not all that young by American standards, say late twenties or early thirties) who is extraordinarily competent and well-read because he is still striving to receive his professorship; the formerly promising young man who has turned into an incompetent blusterer, rhetorician, and public figure in his society; and a very few dedicated economists who have continued and built upon their youthful interest in scientific economics and research, often by moving out of the university environment into a research institute. (Of course, a European research institute is frequently merely a re-

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tirement home and annuity source for academics who are no longer able to compete in the main stream of their subject, and find solace in pursuing portentous pseudo-research into non-researchable amorphous topics.)

Despite a certain amount of criticism of my position, I maintain that economics has, in fact, become an English-language discipline. I would cite the career histories of any number of young European economists to prove that they got their professional start, and their comparative advantage over their contemporaries, by means of a shorter or longer visit to the United States, where they came into contact with real scientific economics. I personally wish that this were not so, that Europe could generate graduate training of a kind that could rival the United States not only in technique but in insight into economic problems. The technique is easy—any intelligent young man can learn mathematics and statistics—the real problem is to learn to observe and try to understand the economic system, not as it functions in one's own country, where one's vision of its functioning may be highly distorted by governmental policies that, as a citizen, one does not question, but as it functions in general. For that, one has to go to the United States, the only country where government policies are still not regarded as sacrosanct and where a man is not only free but encouraged to think of government policies as arbitrary interferences rather than as unquestioned national necessities.

Of the three factors mentioned earlier as influencing national styles in economic research—national culture and the economist's relation to it, university tenure and promotion systems, and outside funding for research—this paper has stressed the first two, though it has also paid attention to the third. In concluding, it is relevant to remark that concern with research is an extremely modern phenomenon in the history of the academic community. For the vast preponderance of that history up to modern times, what was valued was knowledge or "scholarship"—the possession of knowledge—not the capacity or determination to discover new knowledge. Different national styles, at least with respect to economic research, may be associated with each nation's position on the path of transition from valuation of a man for what he knows, whether or not that knowledge is useful and communicable, to valuation of a man for the new knowledge he can produce and communicate, whether or not he is a reputable scholar or a civilized member of the elite. From a social point of view, and in general terms, it is by no means clear that the latter standard of judgment is a better one than the former. Nor is it clear that the emphasis in recent years on high-technology scientific research in economics is independent of the fact that, as a result of the various intellectual revolutions of the 1930's and the interruption of the war years, economics was left with a vast amount of unfinished business, the execution of which, incidentally, was greatly accelerated by the introduction of computer technology. It may well turn out, in the near future, that the possibilities of acquiring useful new knowledge by further research along scientifically respectable computer-and-mathematics-oriented lines will play out, and that the world will discover that what it needs, after all, are political economists and not economic scientists.

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S. E. LURIA

On Research Styles and Allied Matters

To EVALUATE various situations as more or less conducive to scholarship and research one must first define a relevant value system. This is a complex task involving many considerations external to research itself.

I have tried, in this article, to assemble some thoughts on the interplay between researchers and their environment with regard to what might be called research style, and to draw some conclusions concerning the opportunities, pitfalls, and responsibilities that face researchers—be they scholars or scientists—depending at least in part on their particular style of research performance.

Style in research, that is, the way X or Y or a set of Z individuals does research, reflects all sorts of variables: the subject matter; the personality of the practitioner and the influences on him at a given moment; the institutional setting, in terms of everything from the physical vicinity to the political and societal scene; and of course the time period in the history of the discipline and of the world. The same individual will generally not "research" in the same manner at twenty as at sixty; at the University of Southern California as at Oxford; under Franklin D. Roosevelt as under Richard M. Nixon. Style can be influenced by fashion, that is, by records of success. Fashion may foster rapid progress in a profitable direction, attracting a certain type of researcher, or it may turn into a fad with different results. It may stimulate individuals to reach for the stars or to settle for something readily saleable like, for example, pointless "space science." Finally, style may be a response by a set of researchers to the specific conditions of research support under which they work.

In order to draw some lessons concerning the consequences of style for the researcher himself and for the research establishment as a whole it seems useful to illustrate the various factors at work by specific examples, preferably drawn from different fields. I am a biologist with some experience of association with physicists, and a little knowledge of research styles and attitudes in other fields gained through my occasional contacts during thirty years of teaching and research in American universities.

A convenient point of departure is the contention[1] that the humanities are essentially a moral study which can educate people for living as adults—which I take to mean that research in the humanities is an inquiry into the rules of

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behavior that men have devised and followed or broken, and why, and what followed. If this interesting concept is accepted, the humane scholars are immediately removed from the ivory tower and immersed in a situation as full of normative opportunities and responsibilities as is the most engaged of social scientists. In my experience it has not been too rare to find humanists—philosophers, historians, or literary scholars—who draw from their subject matter the elements of a dynamic morality by which to live. In outstanding cases, the scholar may actually live a life entirely permeated by the lessons he or she derives from the pursuit of scholarship.

In this rather crowded life it would not be surprising if I had found no chance to acquire first-hand knowledge of how scholarship in the humanities is carried out. But by a lucky chance my life did afford me an excellent opportunity to see at close quarters top-notch scholarship in action. In my bachelor days it was my good fortune to share a home with a colleague who, I am told, is today the greatest living Shelley scholar. Although I had been hardened and spoiled by a good medical training, a year in Fermi's institute in Rome, two in the Curie laboratory in Paris, and then the milieu of American genetics, I readily sensed in my scholar friend a different style of excellence—an atmosphere not wholly unlike that of the best laboratories, but permeated by a unity of intellectual involvement that laboratories certainly do not have.

My friend's living pattern reflected his research style in many ways: several hours of methodical scholarly work—apart from teaching chores which he did with a sort of disdainful good humor—a late afternoon nap, drinks in studiously chilled glasses, a substantial meal with a touch of 18th century heartiness. Then we had reading and conversation: politics and literature and campus gossip, all distilled in a robust human essence, Swift rather than Boswell. And, of course, intense participation in the socio-political sphere—as intense and vigorous as that of Dean Swift himself. This description is not just gossip; I am trying to convey the sense of a life style in which the values of scholarship permeate the entire atmosphere around the scholar: a total immersion of the scholar into the human enterprise in the light of his scholarship. Nothing could be farther from the ivory tower—that last refuge of the ineffectual.

How frequent among humanists is this integration of their humanism into their humanity I do not know. I have sensed a similar fusion of scholarship and life style in the historian Henry Steele Commager. A small anecdote will illustrate what I mean. Commager and my wife were part of a small group who presented to the U.S. Department of Justice a petition in support of Benjamin Spock and his "co-conspirators." When an Assistant Attorney General asked: "What business brings you here?" Commager replied: "We are doing a bit of American history."

This sample of two is small and rather exceptional. At least for these people, I believe, the style was not the consequence of success—more likely the cause. The fusion of scholarship and active life into a single enterprise may be the key to a style of excellence.

The style of social scientists is affected by the fact that the social sciences

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are by tradition and by necessity less consensual than the natural sciences. On the positive side, the traditional concern of social scientists for socially relevant problems has helped to create the prevailing humanitarian climate of the American university—dedicated, serious, experimental and compassionate. Furthermore, the very participatory nature of their subject matter tends to immerse social scientists into societal affairs at many different levels. Thus, in their style as concerned humanitarians, they often emerge as critics and rebels[2]—as critics of the way things are happening in society at a given moment, as rebels against individual episodes of injustice, or, much less frequently, as radical questioners of the legitimacy of the establishment.[3]

On the negative side, the compulsion to justify their label as "scientists" has encouraged a style of work peculiarly preoccupied with the mathematical tool, sometimes with inadequate appreciation of its limitations. A second compulsion is engendered by the unwritten tradition that success means the production of one's own "model" or theory, often very early in life. Thus many so-called theories expounded by social scientists are not really theories, that is, synthetic formulations leading to hard testable inferences, but rather guesses or hypotheses suggesting possible connections and correlations. The danger is that such hypotheses can, without the careful research they require for validation, become tools for manipulating delicate and even desperate human situations such as school programs for white or black children or the "relocation" of populations in Vietnam.

Is the possibility of fusion of scholarship and practice also available at least in principle to the researcher in the natural sciences? In other words, is there available to the research scientist, besides the potential social consequence of the knowledge that emerges from his work, a normative aspect that influences his style of work and of life? In contrast with humanities and social science, which deal directly with the activities of human beings, the "hard sciences" are primarily consensus disciplines, in which disagreement is usually limited to alternative interpretations of well-established facts, and most theories lead to working hypotheses operationally provable or disprovable.

Some divergence arises, of course, from peculiarities of personal style, from, for example, what have been called the "gee whiz" and the "so what" attitudes. Wolfgang Pauli, one of the great physicists of this century, was supposedly the greatest "so what" physicist of them all, tending to reject any new evidence when first presented—a characteristic often painful to his students. A university is likely to be a happier place for the "gee whiz" scientist than a government or industrial laboratory. He may find it easier to take the "so what" attitude of a colleague than that of a general or a vice-president in charge of research.

Another element of style differs from the divergence of attitude indicated by "gee whiz" and "so what," although it belongs to the same class. Let me call it artistry—the sense of rounded perfection, which is by no means essential in good research but adds something to it. Some years ago I went to a lecture by a famous chemist whom I had never heard. I thought I could spare an hour,

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despite my lack of interest in the subject of the lecture and in chemists in general, because the man was supposed to be a great speaker. The lecture lasted two hours, was highly technical, yet not for a minute could I stop listening and thinking and playing mental games with the speaker. It was artistry, in the work and the presentation alike. To listen was like looking at a sculpture by Brancusi and asking what it was that the artist had added to that piece of stone. What is common to art and such science is the concentration on creation rather than discovery—on a Gestalt goal of totality and harmony and simplicity.

An increasingly felt influence on research style today is one which derives directly from the scientist's normative place in society: the relation of ethics to scientific research. The communal nature of scientific research enforces an integrity in handling factual data, a condition which has been said[4] to impose a habit of mutual respect and personal reliability. A scientist's dedication to an "ethics of knowledge,"[5] to knowledge as a human goal in itself, has also been said to provide a touchstone and a baseline for a style of life. Yet these conditions, stemming from the process of research, even if they are real, do not seem to provide a sufficient set of influences to determine the actual practitioner's way of life. The pursuit of scientific research varies depending on external circumstances, with regard not only to the contents of research but to the way it is carried out—to its style. The charming snootiness of physicists as intellectuals, for example, did not survive the pressure to associate with the military crowd during the 1940's; and some of us who in the 1930's used to look up to physicists as near supermen, feel that the world of physics has never been the same since—physics has been "engineered," not in content, but in style. As a result, the distinction between basic research and applied or developmental research has been blurred and one wonders what consequences this has had on research performance.

Basic research concerns itself with knowledge and understanding, not with specific end products. It requires competence and imagination, not efficiency. Despite the need for financial support, to be obtained preferably through the judgment of a peer group, basic research cannot be programmed to deliver specific results at a specified time. But when research becomes part of a program sponsored by a product-minded agency the style of thinking and working changes and becomes more developmental, as the researcher's interests become fully identified with the practical goals of the sponsoring agency.

Till recently, biology was exempt from such conflicts. Most biological and biomedical research was either basic or related to health or to agriculture. Social consequences, related to the imbalance among various parts of the world in public health progress, agricultural development, population growth, and capital formation, seemed to be quite distant from research and could be written off as a transient result of introducing technology into an imperfect world. The style of the laboratory worker was hardly affected. Recently, however, things have changed. A medium-big scale, not quite that of physics, but relatively substantial all the same, has overtaken biology. Some biologists have

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joined the jet set, and reports—less carefully documented than articles—are written in the first-class sections of airplanes. At the same time, moral anxieties mount. The potential misuse of genetic and biochemical techniques to produce biological weaponry or human degradation are pointed out by scientists truly concerned with warning society of dangers inherent in biology and often guilt-ridden that within a disturbed society their research activity is being so undeservedly sheltered. These worries influence research style, research performance, and also the life style of the scientists in many different ways.

Not least among the anxieties of researchers is their new pattern of associations. Of greatest impact on research and scholarship is the association with the centers of power. In a capitalist democracy like the United States, this means industry and government, including the military. This association was established in World War II because the government needed scientists as well as social scientists and scholars. The relation has since continued, with scientists becoming more and more dependent on the government for research support. The main consequence for research style is related to how the support is decided upon.

The availability of ample research funds from government and foundations creates an entrepreneur system with either the university or an individual researcher as entrepreneur. A new style enters the picture: that of the Mr. X who is an expert in finding out, not who will pay for what X and X's institution *want* to do, but who needs and will pay for something that X knows how to do (or, more often, how to direct his team of technicians to do). One should distinguish, of course, between an opportunistic style of entrepreneurial activity and the legitimate one, which takes advantage of societal interest in certain areas of practical knowledge—environment, population, cancer—to secure funds for good research valid in its own right but also related to the supporting program. Such wise utilization of opportunities has no corrupting influence either on the content of research or on its style.

But the entrepreneurial system does lend itself to opportunism. Insofar as it resembles a competitive production system, graduate students become employees and project directors become fund raisers. A subtle change in ethical standards follows: not necessarily a loss of integrity, but a shift of responsibility from the scholar to the entrepreneur. One sees signs of such a change taking place in biology, in which substantial research support dates only from two decades ago. For example, if someone published some good work, other scientists used to allow him to develop it alone at least for a few years. Now eager researchers rush back from professional meetings to perform the obvious experiments that a speaker had not yet had time to do. Nothing strictly unethical, of course—not according to the ethics of competitive enterprise.

In addition to the entrepreneurial activities of scholars and their institutions, a more intimate relationship is developing between scholars or scientists and what we may call the power establishment, which in the United States consists mainly of government and big business, including the military-in-

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dustrial machine. Here the scholar risks being coopted willingly or unwittingly by the power machine—a misadventure that can befall a humanist or a sociologist, a physicist or a physician, or even a biologist. At this point not only his research style but also his role in society changes. He risks changing from a searcher or a user of knowledge to a manipulator of knowledge in the interest of some power group. This kind of servitude is clearly different from the legitimate service a scholar may provide by giving technical and professional advice. The danger is that a man may come to identify his scholarly function with the specific goals of certain sectors of the power structure. Often at this point confidentiality and secrecy enter the picture and with them an entirely new style—the style of the "insider," who supposedly knows but cannot tell.

A less specialized but more common type of insider in the circles of power is the social scientist, because of the natural and legitimate concern of social scientists for public affairs. Especially in times of widespread political disagreement a type of social scientist emerges who not only advises the government, but also identifies himself with specific policies of some segment of the power structure and with the manipulation of research techniques in the service of those policies—thus perverting the very essence of research yet still claiming the lack of bias of the scholar. The Vietnam war has provided numerous examples of such associations and of the research style that claims a double standard, of academic integrity and of the *raison d'état.*

How does the relationship of researchers to the power centers of society influence the way in which specific research is performed? The size of their research enterprises has made both social and natural sciences dependent on governmental appropriations. This dependence may at times have made researchers and their institutions conscious of vulnerability and therefore cautious in their personal or collective expressions. Fortunately, the content or quality of basic research do not yet seem to have suffered serious damage, at least in the western democracies. This is due mainly to the sound policies of some of the agencies through which support has been provided—the National Science Foundation, the National Institutes of Health, and the Atomic Energy Commission in the United States; the University Grants and the various Research Councils in Great Britain; and the Centre National de la Recherche Scientifique (CNRS) in France.

The last named is an interesting experiment. Spurred by Paul Langevin and Jean Perrin, in 1938 the Front Populaire Government of Léon Blum established the CNRS. It remains, I believe, the only successful attempt in a non-socialist country to create a system for careers in research outside the teaching profession. Until recently the main drawbacks of the expanding CNRS have been that it tended to disperse funds and to alienate potential teachers from the desperately understaffed and overcrowded French university system. Yet it has obviously flourished and achieved some of its original goals, partly because most CNRS laboratories have been located within the universities, with substantial mutual benefits. A similar experiment in Italy after World War II became

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hopelessly entangled in the byzantine politics and medieval structure of the Italian university, and proved utterly unproductive.

This brings me to the next aspect of the influence of environment on research, that concerning the role of the university. With the coming of big science—big humanities are still a few years away—the question is whether the university, specifically the American university, is an optimal or even an appropriate setting for the pursuit of research. The supposed need of researchers to be sheltered from alternative duties, the uneven quality of different universities, and the mammoth size of some of them may suggest that research could be done more effectively elsewhere. This suggestion should be examined very vigorously. The coupling of research and teaching provides irreplaceable benefits to both enterprises. Education at the college and graduate level is dwarfed if it cannot expose students to serious practitioners of scholarship and research. Even scholars not in the forefront of their fields can make available the knowledge of how research is done—of how knowledge is acquired. There may be effective teachers who are neither scholars nor scientists; but is a student educated for life in the present world if he or she has not been exposed to the process of dynamic on-going research?

If education benefits from its relation to research, research benefits even more. Research is essentially an historical process, transmitted not only by the printed page but also by direct personal contact. The university provides a flow of young talent that is never available to an isolated research institute. Students new to the field provide an invaluable stimulus to exploring areas and by-ways which the established investigator, too tired or too busy, might neglect. The interaction of researchers with students within a university keeps the research enterprise "open," especially in times when the dependence of research on sponsoring agencies may tend to produce a closed style of functioning. In other words, the style of research is modified by the presence of students.

Apart from the question of students, the university extends another benefit to research: because of its traditional standards of openness and academic freedom the university community tends to discourage elitist or partisan or profit-seeking misuses of the products of research. Academic freedom is the great invention of the university. It protects, more or less effectively, not only the right of the researcher to do what he wants to do (provided he obtains the means to do it), but also the right not to do what he disapproves of and to speak out against it. It protects—again with varying efficiency—the scholar's right to behave as he pleases within or without the university even if he offends the tastes and convictions of others (but not, of course, their personal rights). This freedom, essential not only to research but to its role in society, would of course be meaningless unless tied to that much maligned institution—academic tenure.

Academic freedom is a major determinant of style in research. Historically, scholasticism and authority in research were defeated by academic freedom, and it was the universities that led the fight, with varying depths of conviction

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and varying degrees of success. It is, of course, a war that never ends: the university is an integral part of society and, like all other parts, is beset with contradictions. Within the university, freedom of research is limited only by questions of financial support or of opportunities to get published. Research in the university is free to the extent (or perhaps a bit more than the extent) that the university itself is free. The university's inevitable responsiveness to dominant social forces, its willingness to promote such forces, or submit to them, or compromise with them, or oppose them, whether in the name of social justice or of intellectual freedom, influences the climate and the style and the contents of research. These days one often hears the university attacked either as a submissive tool of the establishment or as a politicized minion of the anti-establishment—as the strumpet of the right or as the strumpet of the left. What these criticisms ignore is that, to the extent that the university is a community of scholars, it must be one of the main fields on which the ethical battles of society are fought. University research may reflect by-and-large the concerns of the establishment, but the university is also a place where radical intellectuals can insist on the opportunity to carry out unpopular, anti-establishment research. Within the American university, a certain amount of radical social research has long been sheltered, provided it did not make too much trouble. The so-called politicization of universities, so bemoaned by the vocal defenders of the ivory tower, has in fact revealed the extreme smallness of the radical research enterprise and the urgent need to foster it, not as a political tool but as an integral part of the pursuit of knowledge.

The university has this to offer to the research enterprise, that it is a powerful and traditionally proud institution: probably the only institution in the history of mankind that has been molded by intellectuals for the promotion of the intellectual enterprise, not at the behest of the community as client, but in the sturdy belief that it can best serve by determining its own direction. Hence the university provides the natural home for research on controversial subjects, and it is up to researchers to make optimal use of such opportunities.

Finally, there is a crying need to educate the general public about the nature and content and goals and value of research, and the university is a natural place to begin. Perhaps researchers are not the best qualified nor the most dispassionate educators with regard to their own work. But someone has to do it. Professional writers, among them some brilliant popularizers, often fail to do the job because they try to cater, like all mass media, to what they think the public wants to hear. It is sufficient to read two of the best newspapers in the world, the *New York Times* and *Le Monde*, to find that the great majority of "science" stories are stories about technology. Technology, of course, is not science: it is a product of science, although its practice by engineers and technicians requires scientific training. Probably similar misunderstandings develop between basic humanistic and social research and their applications. This confusion, nurtured by the mass media, insufficiently clarified by school teachers, and encouraged by scholars either through lack of interest

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or through willingness to exploit the ambiguity, is a handicap to the scholarly enterprise. Science is being accused, for example, of being the source of evils that are really misapplications of technology, over which scientists very seldom have any measure of control. Scientists respond in a variety of ways. Some plead guilty to lack of responsiveness to human needs and even to irresponsibility in the pursuit of their calling. Others insist on the right to be indifferent to the consequences of scholarship once it ventures outside the ivory tower. And still others claim that science can do everything and anything—if only it is properly funded. This variety of responses further blurs the issues for the public and adds to the confusion.

The increasing complexity of science on the one hand and the increasing gap between humanistic scholarship and mass education on the other hand, call for a renewed dedication to education. The public should be told and made to understand what research is, what researchers do, and what research produces, so that an informed public can judge what research and its products, including properly trained personnel, are worth to society. This information need not be imparted categorically or apologetically. If research is part of the human enterprise, an informed society should be given the knowledge it needs to judge it intelligently before it makes a decision whether to press for alternative priorities. It should be possible to inform the public of the potential consequences that research may have when developed by technology. The public also needs to be made aware, however, that research generates technology only through a complex process in which societal, nonscientific forces intervene; that decisions as to whether to develop a technology and how to use it are only minutely influenced by science. The facts that a new toxin, for example, can be stockpiled for use as a weapon, or that nuclear fusion can be used in hydrogen bombs do not mean that microbiology and nuclear physics are branches of weaponeering. This line of reasoning does not, however, exempt researchers from the responsibility of making themselves aware of the possible applications of their work and of educating the public accordingly.

Legitimacy is claimed for research on the bases both of its contributions to knowledge and of the material or moral benefits accruing from that knowledge. This legitimacy needs to be asserted at a time when the failure of certain expectations of progress and the misuse of certain fruits of science have turned young people all over the world against rationality itself as a source of valid knowledge. The rationality embodied in research remains the foundation upon which mankind must strive to build its uncertain future. Nevertheless, the pursuit of research may well have to be accompanied by more humble claims and by greater sensitivity to the need of society to understand, choose, and evaluate the social contributions of the research enterprise.

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JOHN C. ECCLES

The Discipline of Science with Special Reference to the Neurosciences

I HAVE CHOSEN to center my contribution on the neurosciences because they and the related philosophical problems of brain and mind have been the concern of my whole intellectual life. However, from this base I will also make some more general comments on the biological sciences. It may be of interest if I begin with an outline of the rather wide experiences of my academic life.

Briefly, my scientific life began at Oxford in 1925 where for several years I was a pupil of Sherrington and where I also had the rich experience of college and laboratory life for twelve years. Then, after six and a half years directing a small research institute in Sydney, I again resumed university life in New Zealand for eight years. There I had an intensive teaching and research life in a small university. After that rather limiting experience I was fortunate to have for fourteen years the opportunities of a new and exciting post-graduate University—the Australian National University at Canberra. Finally I left for America, and after a disappointing two years in a research institute at Chicago, I now have had four years at the State University of New York at Buffalo under conditions comparable in many ways with those I enjoyed at Canberra. Thus, in addition to my initial medical course at Melbourne University, I have had the experience of researching and teaching in four universities and in two research institutes. Besides doing my share of university administration, I have served on government scientific committees in New Zealand and Australia, and on scientific advisory committees of the World Health Organization and of the International Brain Research Organization. In America I have served on the scientific advisory committees of the Muscular Dystrophy Association and the Paraplegia Foundation. I was for four years (1957-1961) President of the Australian Academy of Science. I have been an editor of several scientific journals, and have had the experience of organizing international scientific symposia.

Because of my experience of science in many countries and of a continuing interest in the philosophy of science, I have developed strong opinions on many of the questions of concern in this issue. I will of course be looking at the subject of research from a very different standpoint from that of big physics. Fortunately nothing in biology is yet comparable with the situation in physics, and most biological research is still carried out under conditions which would

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have been usual for physics until the 1940's. Most efforts to build biology up into something equivalent to large scale physics seem to me to have been disappointing and I think misguided. I believe that in biology we are still in the enviable position of being able to work as creative individuals with, of course, small teams blending those special talents that are required in technical and theoretical attacks on fundamental problems.

Science and Technology Contrasted

I would define science as the systematic attempt to understand and comprehend the natural world. It is a determination to enter deeply into the natural world, not just superficially, but to achieve a rational expression in language and mathematical symbolism of the order and beauty of operation that lies behind all natural phenomena. This is what the aim of science is. Its scope is not just the external world. The scope of science includes ourselves. By that I mean that all aspects of our own experiences of ourselves—our perceptions, imaginings, emotions and actions—everything properly comes into the purview of science.

Now this enterprise of science is dependent on the imaginative and disciplined activities of unique individuals who ideally form a free and supranational society. This essential feature of science certainly is not fully understood and appreciated by the politicians and the bureaucrats who operate even the most liberal political systems, and in most countries it is not understood at all. Correspondingly there are not very many countries in the world where science flourishes in a free-flowering manner. There are far too many countries where science does not grow at all or where it is extremely maimed in its growth.

I had not fully recognized that so many countries were impoverished with regard to brain scientists until a world survey of brain science was done for the International Brain Research Organization of UNESCO. In some countries of up to a hundred million people, there are no brain scientists at all. There are several countries which offer generally unfavorable conditions, but which have experienced some isolated growth and flowering due to the courage and genius of one leader and his devoted disciples. As an illustration from the past, I give Ramón y Cajal, who late last century in Spain, with practically no support at all, became the world's greatest neuroanatomist, building up a world famous school and making a magnificent scientific contribution that to this day we still much depend on. There has so far only been one Ramón y Cajal in the sciences of the nervous system. He exemplifies a very rare phenomenon: a high level of scientific performance achieved by a genius under unfavorable conditions.

It is important to draw a sharp distinction between science and technology, or applied science as it is sometimes called. I am not going to say which has the higher status, but it is necessary to be clear about these two categories, although the same person can be both a scientist and a technologist. He can wear, as it were, different hats for different occasions, but what he does on these two occasions is quite different. There are technologists in many countries where

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there are no scientists. In fact all countries have technologists; even countries in the Stone Age, for example, have experts in the manufacture of stone axes!

The aim of the technologist is to apply scientific knowledge and empirical knowledge in some useful way. He may be designing and constructing scientific equipment. I am a technologist when so engaged. Technologists are responsible for all the marvelous inventions that have transformed the conditions of our life. Think of the spectrum of revolutions in the means of communication, in the materials for all purposes, and in electronics; think of all the new discoveries and inventions relating to medical practice, to agriculture and food, to the chemical industry, and to computers. This human activity is quite different from that of science, although it utilizes or even exploits the discoveries of science. Of course, a technologist has to have a wide knowledge, imagination, and high intelligence, just as does a scientist. I am not underrating the nature of his performance; I am just saying that it is different from that of the scientist. The difference is a difference in objectives.

The scientist tries to understand or comprehend the natural world as he experiences it, using for this purpose hypotheses which are tested under specially designed experimental conditions in which most elaborate scientific instruments are often employed. Yet, in the end, no matter how complex the apparatus is, the information that it delivers has to be looked at and observed by a scientist, who must examine it critically in relation to his hypotheses, and then, perhaps, reformulate his hypotheses. The essence of what a scientist does is to imagine and to explain what lies behind phenomena, in other words to try to comprehend the natural world. On the other hand, the technologist is utilizing already confirmed knowledge about the natural world for practical purposes.

The Nature of Scientific Investigation

I now wish to give some understanding of the nature of the society most favorable for the creation of a scientist. It is neither a dogmatic society nor an authoritarian society. It is a free society of people adventuring into the unknown. Following Sir Karl Popper,[1] I can state that it is of the utmost importance for scientists, and particularly for the leaders of scientific research, to be illuminated and guided by a theory of scientific method. I realize, of course, that most scientists are not concerned with the philosophical basis of their work. They would regard this as of no value in their efforts at scientific discovery. Many have some naive belief that science is concerned with the making of scientific observations with the best techniques available, and that out of all these observations there will emerge some coherent story or hypothesis, which is often rather apologetically referred to as a "provisional hypothesis." This term is, of course, a device to protect the scientist from identifying himself with some hypothesis that may bring him into obloquy if it is later disproved. But implicit in this use of the term "provisional hypothesis" is the concept that, as the scientific investigation in question unfolds, it will become possible to formulate a full fledged scientific hypothesis or theory that has evolved beyond the provis-

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ional stage to take its rightful place as an integral component in the body of scientifically established knowledge.

This misunderstanding of the nature of science has serious consequences both to science itself and to individual scientists. Scientific publication becomes too much occupied with the reporting of experimental observations that achieve status because they have been carried out by the latest and most expensive techniques and not because they are designed to test some particularly interesting scientific idea or postulate. As a consequence, scientific literature is overwhelmed by mere reportage of observations published just as observations without being organically related to precisely formulated hypotheses. Such observations are scientifically meaningless. They are boring and soon forgotten. There is thus great waste in every aspect of scientific endeavor—in the efforts of the scientists themselves, in the deployment of scientific equipment and technical personnel, and in the use of facilities for scientific publication.

Besides this waste there is a real danger that this misunderstanding of the scientific endeavor may have serious consequences on the scientist himself. The erroneous belief that science eventually leads to the certainty of a definitive explanation carries with it the implication that it is a grave scientific misdemeanor to have published some hypothesis that is eventually falsified. As a consequence, scientists are often loath to admit the falsification of their hypotheses, and their lives may be wasted in defending what is no longer defensible. Whereas, according to Popper, falsification in whole or in part should be the anticipated fate of all scientific hypotheses, and we should even rejoice in the falsification of an hypothesis that we have cherished as our brain-child. Such a belief relieves the scientist from fears and remorse, and science becomes an exhilarating adventure where imagination and vision can safely lead to conceptual developments transcending in generality and range the experimental evidence. The precise formulation of these imaginative insights into hypotheses opens the way to rigorous testing by experiment, always with the anticipation that the hypothesis may be falsified and replaced in whole or in part by another hypothesis of greater explanatory power. In this way conceptual developments lead to experimental testing which is always designed in relation to hypotheses. The status of a scientific hypothesis should be measured by the effectiveness with which it lends itself to rigorous experimental testing and by its explanatory power, which should go far beyond existing knowledge; its status, in other words, should be measured by its predictive scope.

Descriptions of Popper's views on the methodology of science tend to give an impression of a rigidity of operation which rarely corresponds to the experiences of scientists—even of those who, like myself, fully subscribe to his methodology. As I examine my procedures in the course of making some scientific discovery, I find that they look much more like the opportunistic tactics of guerrilla warfare than like the organized strategies of major battles.

My experience is that often, at the beginning of a particular investigation, I have only a very vague horizon-of-expectations (to use Popper's felicitous phrase),[2] sufficient only to guide me in the design of my experiments; that way

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I am able to maintain flexibility about concepts which are developed in the light of the observations, so that my horizon-of-expectations is greatly advanced and developed and itself suggests much more rigorous and searching experimental testing than could have been designed at the outset.

The Concept of Scientific Leadership

I feel that one cannot stress too strongly the importance of the elite in the various branches of science. One can observe the rise and fall of different branches of the neurosciences in a country according to the kind of leadership that is provided. I am not of course claiming that without elite leadership nothing can be accomplished scientifically. There could well be competent science, but it would be unimaginative. So, of course, is the great bulk of research work carried out in any country. It is uninspired, but nevertheless valuable because it fills in gaps in the knowledge of a subject. Without an elite, there is little exploration of new and fruitful lines of development. This is particularly evident if one acts as an editor of journals or goes to many scientific conferences. Without the leadership of the elite I think that there is a danger in some fields of investigation of becoming bogged down in routine procedures. The scientists seem to get into some rut of technical performance, where they competently carry out research projects in a field which is entirely familiar to them. Of course they work in a field in which they have all the equipment available in the laboratory and all the technical and experimental know-how; nevertheless this situation does not make for the good training of young investigators.

It is for this reason that I strongly advocate the encouragement of travel with research fellowships so that the young investigator can go to superior laboratories in any part of the world. These laboratories need not be in the great and wealthy countries of the world. I could name several in Australia for example. I can give an example from the neurosciences of a remarkable group in a small country. I refer to the Institute of Neuroanatomy at the University of Oslo under the leadership of Jansen and Brodal. In the field of neuroanatomy the research of this school has set world standards for at least three decades and is continuing to do so. Investigators from many parts of the world go there in order to be trained and to return to their own countries with the intellectual advantages of being associated with a distinguished institute. I could give, of course, many examples of institutes in the large highly developed countries that have spearheaded investigations in various aspects of the neurosciences.

The more one surveys the whole world field, the more one is sure that an early period of association with an elite group is essential for the development of youthful investigators. I can speak about this personally because I had the lucky opportunity to go to Sherrington at Oxford in 1925 and be there in what I consider to have been the foremost neurophysiological laboratory in the world at that time. Thus for a number of years at a critical stage of my life (when I

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was twenty-two to thirty-four years old) I was associated with youthful research workers from other countries who had come to work in the Sherrington group, workers such as Granit, Denny-Brown and Fulton. And at the same time there were two other centers in England that were in the top world class in the neurosciences: Adrian's group in Cambridge and Dale's group in London. Verily it was a wonderful experience for me to be in England from 1925 to 1937.

It is of course true that countries rise and fall in scientific eminence in one or another field. For example, in physiology and the neurosciences, Germany and France were preeminent throughout most of the 19th century. There were such world leaders as Ludwig, Helmholtz, duBois Reymond, and Claude Bernard. But in the later part of that century there was a wonderful flowering in Britain, particularly at Cambridge, though its leaders had been nurtured on the Continent. Gaskell, Langley, Sherrington, Starling, Bayliss, and Schaefer were already famous by 1900, and the tradition was continued into the early 1900's by the remarkable galaxy of Dale, Lucas, Hill, Adrian, Lewis, Haldane, and Barcroft who in turn fertilized yet another generation of world leaders. Finally, after the Second World War, the United States gradually took over world leadership thanks largely to American scientists who had had the advantage of a period of training in Europe or who were immigrants from Europe. From the 1950's on there has been a similar transplantation of science to Australia, but on a much smaller scale. In the last ten years, more than half of the Nobel Prizes in science have been awarded to Americans.

The Training of Scientists

I think that scientific elites, if I may call them so, could be more effectively used than they are at present. With modern techniques of communication, such as those provided by the various kinds of audiovisual techniques, it should be possible to distribute their unique scientific abilities and performances more widely than is done at present. Furthermore, it should be a moral obligation on the part of the elite to cooperate as much as possible in such an enterprise. What I am trying to suggest is that those scientists of proved performance and influence should not restrict their teaching to formal lecturing in their institutes and to discussions with their own personal research groups. The study of the way early experiences have formed leading scientists amply demonstrates how vitally important it is for scientific leaders to light up the youthful enthusiasms of potential men of genius.

In the past such contact has been very much the result of lucky chance. The great Ernest Rutherford began as a young unknown physics student in Canterbury College, Christchurch, New Zealand. His only chance was to go overseas from this isolated limited environment and to work under a great master. He almost missed this chance. He was placed second in the award rating for the only overseas fellowship from New Zealand. By good fortune the ap-

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pointee, a geologist, chose marriage and so could not afford to travel! Thus it came about that in the 1890's Rutherford was in Cambridge working at the Cavendish under the supervision of J. J. Thompson, which gave him the best opportunities in the world! Of course now the situation is immensely improved for young students in remote countries like New Zealand and Australia, both with regard to local facilities and to the opportunities for travel. But it is still almost hopeless for students in Indonesia, Southeast Asia, Africa, and many South and Central American countries.

I now come to the question of how the scientific elites can best communicate with the young students in their field. It is important that this communication be as widely dispersed as possible and convey the scientific personality of the expert. For example, his imagination and creativity should shine through the printed text. Books giving the sense and style of lectures are very important. Furthermore, such books should carry the sense of adventure and excitement that one sees in Watson's *The Double Helix*[3] or in Monod's *Chance and Necessity*.[4] It is unfortunate that *The Double Helix* carries the blemish of an obsession with personality traits; otherwise it would be a model of the kind of book I am proposing. I am at present engaged in writing a book in which I am attempting just what I am advocating. It is based on a course of six lectures, the Patten Lectures at Indiana University, on the theme "Communication in the Nervous System," and will be published under the title *The Understanding of the Brain.*

Besides books, though, we have today a very important means of communication in the audiovisual media, particularly in special TV programs. We have already, for example, a TV program by the BBC called *The Mind of Man,* which was a very valuable scientific display, though of course it was aimed more at the general public than at the young academic. TV can also be used to show interviews and I have had several recent experiences with this. Interviews on cultural programs certainly do convey to a wide audience the particular personal approaches and beliefs of the participants. Finally, I think much more could be done to develop special TV filming of lectures by scientists gifted in exciting communication. To be successful these lectures should combine histrionic display with copious illustration specially prepared. They should seem to be spoken spontaneously as if the thoughts of the speaker were clamoring for utterance. The reading of prepared manuscripts should be discouraged. Lectures of this kind convey something hard to capture in the printed word and I have a feeling that they should be studied and developed by specialists in audiovisual aids.

In summary, I would say that those of us who have achieved "elite status" in our fields should try to communicate not only in solid works of scholarship such as scientific monographs and reviews, but also in an informal manner that reveals the personal touch of lives dedicated to science. (I should emphasize that this term "elite" is used throughout in no rigorous or exclusive sense. There are all gradations in elite status.)

In order to provide first-class institutes for the training of youthful in-

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vestigators from all parts of the world, I have for some time advocated international research institutes modeled on CERN in Geneva. In fact the Molecular Biology Organization of Europe is considering an associated institute of this kind which would have a neurobiological component. I favor such an institute because I am deeply concerned at the poor choices students from other countries often make when they select an institute to go to for training. Bad choices can lead to much suffering and frustration. Students from Indonesia, Southeast Asia or many South American countries, for example, find it difficult to obtain reliable information about where to go. In their own countries relatively few scientists have world experience, and even those who do are often many years out of date. It would certainly eliminate many of these poor choices if there were international institutes with a small permanent staff engaged in research and an arrangement for distinguished scientists to visit for a month or so to give seminars and to make themselves available for discussions. The young scientific visitor from overseas could be assured of a very diversified program and the opportunity to discover which aspect of his scientific discipline he would like to specialize in. He also would be able to talk with visiting scientists about possibilities for joining their research groups. I am sure that in the neurosciences it would be possible to set up such institutes, one in Western Europe and one in America, without too high a level of expenditure.

There are, of course, already such institutes to which scientists can come, particularly marine biological laboratories such as that at Woods Hole. In addition, a fairly wide range of opportunity is available in the biological sciences in higher institutes for research such as the Australian National University of Canberra with which I was associated for many years, the Rockefeller University of New York, and the various Max-Planck Institutes of Germany and Medical Research Council Institutes in Great Britain. During the fourteen years I was associated with the Institute of Advanced Studies of the Australian National University, I had the good fortune to appreciate the success of such an institute. In fourteen years, over seventy young scientists from twenty-two different countries came to join the research staff of my small department, and over 400 scientific papers and three books were published. A person usually stayed not less than one year and often for two to four years. There were always far more visitors than permanent staff members. I found it an exciting and rewarding experience in international cooperation.

Research Teams

As I have mentioned already, there are virtually no examples of outstanding success in big team research in biological sciences which compare in any way with those in big physics.[5] Partial exceptions are some biochemical and molecular biological investigations on the genetic code and its role in the building of proteins. Presumably more big team investigations will take place

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as emphasis increases on the necessity for powerful computers as instruments in biological research.

Regretfully, when research is oriented to the machine, as it tends to be when the machine is large and expensive, the machine tends to take over the development of the program. Research becomes oriented to the kinds of service the computer can provide. I find that, in almost all cases in biology where computer programming is dominant, the research has lost its character and become dedicated to the task of programming the computer to extract from the data the kind of message that it is particularly equipped to give. Such work tends to be sterile and dull.

As I have observed the main developments in neurobiology, I have come to see that we are still in a stage where the imaginative ideas of individuals play a leading role and that large programs of scientific research are often opportunistic. This situation arises because so much that is unpredictable occurs in scientific investigations. We must therefore cherish the flexibility that comes from personal control. Each experiment is an adventure into the unknown, and the traces on the oscilloscope screen may give us messages that are challenging and through which, as I like to say when they are unexpected, "nature is trying to tell us something." We have to listen and try to understand in order to penetrate into one more secret. The standard automated computer programming investigations cannot do this. Because the computer does not recognize unexpected messages, I would say that its influence on scientific advance is often mischievous. Of course I do not mean to say that one should not use the very best techniques for any particular investigation. That is mandatory. For example, I have long used computer averaging techniques of quite a sophisticated kind, but I make sure that everything computerized is under surveillance all the time and is preserved on a photographic record.

In my experience the best sized group is two to four people. These can fit into one research room and actively participate in the experiment in hand. All can watch the results and talk about their meaning and what to do next because, as I see it, biological research is still a kind of warfare against the unknown. Research involves strategy and tactics in the conventional military sense, and tactics become the essence of any good experiment. It is essential to make the most of what is being revealed by the experiment in order to give it due significance in the scientific story. This means that during the experiment all kinds of controls have to be thought up and utilized. This is one reason why I am now strongly biased against the lone investigator in the neurosciences. I had my own experiences working alone in the early simpler experimental days. But now I regard experiments on the central nervous system as too demanding physically and mentally to be undertaken by a lone worker. Moreover, such a worker is deprived of all on-going discussion during the actual experiment.

A danger to present scientific development is the tendency for research groups that assemble under an outstanding scientist to grow and grow. The

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head of the group is more and more overwhelmed by administration so that in the end he only sees research second hand. He misses all the excitement and decision-making that are the essence of a successful experiment. He misses the challenge to thinking and the development of new ideas, which are essential for the progress of any scientific investigation. Rarely in the neurosciences can research be fruitful for long unless its leaders are immersed in actual experimental investigations.

It is generally believed that scientists lose their initiative in early middle age. I think that this is wrong. They do not lose their initiative but they are prevented from exercising it by the burden of administration and extracurricular activities that descends on them in this rather over-organized civilization. I have had to struggle to prevent myself from being submerged in a mass of administration and committee work. Somehow I have avoided this fate, partly by changing my university affiliation at appropriate times! Nevertheless, one is continually tempted to accept responsibilities and to increase one's research group. I can give a recent personal experience.

Our research here in Buffalo on cerebellar physiology was going very well, when I was tempted by an invitation from the Sloan Foundation to apply for a grant to develop an additional field in neurobiology. I chose the very important field of neurogenesis in the cerebellum because it related to our own investigations, and I could see a great future in it. Developing it would have meant bringing in additional scientists and doing a great deal of bothersome administration in order to set up two electron-microscope laboratories. Nevertheless I was so attracted by the prospects that I expended much effort producing a grant application detailing the specifications of a research program. I even arranged for two overseas experts to come and take over this research on neurogenesis. I was very disappointed indeed to have my request rejected, particularly as I had thought the invitation was genuine! At least I felt less discriminated against when I learned that many other neurobiologists had suffered similarly! Now I can look back on this rejection with equanimity. I realize that acceptance would have involved me heavily in administrative affairs and much of my present research would have been threatened. So out of disappointment comes good fortune!

The message I take from this is that we should all be careful about the blandishments we are offered which tend to make us accept new responsibilities and grow indefinitely in our research commitments. I continue to believe in small groups working in close association and in incessant discussion.

Freedom and Science

I have talked about science and the atmosphere in which it is conducted; now I ask how freedom fits in with all this. Why is freedom, as we define it, so necessary? Its necessity comes from the tension between the two different forces operating on scientists. On the one hand, scientists must be able to adventure ideologically and imaginatively in order to experience new visions

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and understandings of nature. All scientists, except those conditioned by political masters, believe that only in an atmosphere of freedom can they do this. There must not be some imposed dogma limiting what can be done or expressed in science, as for example, that from which Gallileo suffered. On the other hand, a critical and even repressive control is exercised by the scientific community itself with its scientific societies, journals and more informal groups. They function as arbiters, although they are not properly or even deliberately constituted for this purpose. There is also the on-going discussion and criticism of science by scientists whenever they meet. It is the tension between these two poles of scientific activity, on the one hand, freedom to adventure, and on the other, critical restraint and even repression, that gives science its amazing strength and success. Sir Karl Popper[1] describes these poles as *conjectures* and *refutations*. Very rarely does anything phony survive for long. Scientists may develop and express mistaken ideas, but experimental refutation soon prevails.

In recent times, science has not gone down many side tracks, not the main body of science. (You can, of course, think of the genetics of Lysenko in Russia as an alternative route.) I am not advocating unrestricted investigations in the name of science. I like to think of science as a great garden, replete with wonderful plants, but threatened always by the concurrent growth of weeds. By the criticisms at meetings and refereeing by journals most of the weeds are eliminated or at least controlled; at the same time critics must use wisdom and insight so that strange new fruitful plants are cherished and not pulled out when they are immature and hard to recognize. When a scientist can grow a beautiful new plant that eventually develops in a magnificent free-flowering manner, then he receives, deservedly, the highest rewards in status from the scientific community.

Safeguards against arbitrary action are necessary because in a free society the community of scientists does not act as a single dogmatic authority. Instead there are many authorities, often advocating different and even opposing schools of thought. They do make mistakes, but usually they are not fatal to anyone.

On the other hand, the lack of rigid dogmatism in most fields safeguards freedom of scientific exploration fairly adequately. In the free countries of the West there is also freedom to travel from one country to another for scientific meetings and for scientific training. In fact, travel is greatly encouraged by generous financial provisions. If the young scientist has ideas that do not fit in with those of the professor he is working under, he is free to move to another laboratory with scientific ideas more consonant with his own. He can move because he has freedom to travel, which is very important, and also because there are always places for dedicated scientists. This is why a free society is so important. Even within most laboratories there is room for differences of opinion and even a generous tolerance for dissent.

This freedom to travel internationally does not obtain in the Soviet Union, where only the safest people are allowed out, particularly for their early

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period of training, and where the young usually work where they are told to work and under a master to whom they are assigned. Thus their fate is to continue with experiments confirming the orthodox views of a master. More freedom is preserved in the so-called satellite countries which are really colonies of the Soviet Empire, though to judge from attendance at the 25th meeting of the International Union of Physiological Sciences in Munich in 1971, the situation of these countries is becoming as restrictive as that in the USSR. I know eminent scientists in these Soviet "colonies" who are unable to accept invitations to scientific meetings, even when no cost is involved.

Scientific Publications and Scientific Societies

How do we manage to make progress in science? A scientist does some experiments and gets a lot of results. Frequently before or during the course of publication these scientific results and ideas are presented verbally at some scientific meeting, where the author defends himself against criticism and further explains his results. He writes an account explaining his problem and discussing his results. Ordinarily he goes through the mill of presenting his data to a scientific journal. I am an editor of two journals and an advisory editor of many others, so I am often called to referee papers. Our task is to criticize and evaluate a paper, and also to make suggestions about changes that should be made if it is to be accepted for publication. If an author is unable to get his paper past the referees of one journal, he can try another journal. There is not just one journal, with one set of editors, for each specialized study. There are many journals and a scientist is free to pass his work around, just as long as he does not publish the same paper in two different journals. He can try one after another until he finds one which will take it, and as far as I can see almost everything gets printed eventually! Often papers have to be modified and usually referees give good advice about revision.

Once the paper is printed, it enters the next stage of testing: it is read, sometimes by experts in the field. If you are an expert in the field, at first you look through it very cursorily to see if it is worth reading, and then you decide how much time you should spend with it. Sometimes, you read and re-read it, making copious comments in the margins, and sometimes it just disappears from your mind and even from the literature. Some papers have an extraordinarily short "half life."

The final stage may be remarkable. The scientist may find people not only quoting his paper, but accepting it so fully that they base their own experiments upon it, or even write textbooks or monographs in which the paper plays a key role. This is the ultimate success. However, don't be misled by that. The paper could still be wrong, and probably is at least in part! Even after this initial success it remains open to attack, either directly due to new experimental developments or when it is found to be inconsistent with quite other investigations. And so the scientific process goes on—an interplay of conjectures and refutations.

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I think it is very important that we cherish and appreciate the operational aspects of scientific societies and journals. According to tradition, scientific societies do not intrude on the scientific freedom of their members. Even as august a body as the Royal Society of London does not adjudicate as to whether any particular scientific statement or hypothesis is true or false. Its members can think what they like, but it is the tradition that as a society it never adjudicates. The same is true of the National Academy of Sciences, and of other such bodies.

Thus scientific orthodoxy is not maintained by some formal establishment. It is dependent in a strange manner on informal and often unorganized discussions among small groups of scientists. This is how science works. We are all the time chattering about this or that scientific story or scientist, taking part in the incessant gossip of the laboratory. In fact laboratories should be maintained for this purpose even when there is no experimental program! Students must learn by experiencing it, that this on-going scientific discussion can be quite searching and merciless. Often the most valuable aspect of scientific meetings is the opportunity they give for free informal discussion between the program items. Scientists can be very hard in their criticism of each other, but they can be equally generous and admiring. It is always the level of performance that is being assessed. Often judgments may appear to be uncharitable, but at least it is rare to hear accusations that a scientist lacks integrity. Rather he is accused of being misguided, or of having mistaken ideas or even of not having any worthwhile ideas at all!

International meetings of scientific societies have grown to massive proportions with many thousands of participants, and twenty or more subsections that meet concurrently. Some aspects of these meetings still have scientific value—for example some invited lectures and well-chosen symposia. However their principal value accrues from the satellite symposia held in other places, often widely dispersed on the same continent. These symposia are usually restricted in size so as to give opportunity for effective discussion and personal communication. In my experience these meetings are not thereby restricted to celebrities, but include a good proportion of young scientists.

Meetings of specialist societies at the national level provide excellent opportunities for younger scientists to meet, often for the first time, with scientists with similar interests from other centers. Unfortunately they can become immense, like, for example, the annual meetings of the Federated Societies in America. Even the Society of Neuroscience of U.S.A. had more than 1000 participants at its first meeting in Washington, D.C. in 1971. As a consequence, there were often two different programs of interest running concurrently. Nevertheless these meetings are of enormous value to young scientists. Much effort and originality should be given to improving the programming.

I would like to make special mention of the British Physiological Society as it is in many ways an ideal scientific society. It is informal, yet efficiently organized. It maintains an excellent atmosphere of personal friendliness, yet those presenting scientific papers or demonstrations are subject to the most

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searching criticisms. What surprises visitors are the traditions that these criticisms must be free of personal attack, and that they must be accepted with good grace. Characteristically there is a sporting atmosphere in the way hard knocks are given and received! The society is remarkable for its truly democratic spirit, where the youngest member ranks with the most senior. I have been a member since 1929 and was in regular attendance until I left England in 1937. I still try to attend if I am in Europe, and I am much encouraged to see how the fine old traditions are continued almost exactly as I first knew them in the 1920's. And so we can be assured that the stream of British physiology will flow even more strongly as it continues into its second century.

Conclusion

Science is a very specialized kind of discipline. It flourishes best under special conditions of society. It is sharply distinguished from technology. Science is the effort to understand natural phenomena, motivated primarily by intrinsic interest, not by utility. On the contrary, technology is an effort to apply empirical scientific knowledge to some useful purpose. Science flourishes when there is freedom to exercise to the full the two aspects of the scientific method: on the one hand, creative imagination must seek to build hypotheses of the utmost generality extending beyond existing knowledge; and on the other hand, experimental investigation must subject these hypotheses to the most rigorous empirical testing.

Science continues to flourish in a country to the degree that it has leadership from what we may call the scientific elite. Their influence is essential for the training of the next generation of scientists. Much more attention should be given to the high level training of the next generation of scientists. I have given a brief account of the manner in which this can most effectively be accomplished, namely by a type of apprenticeship in which young scientists work in daily contact with small laboratory groups. Scientific method in both its experimental and theoretical aspects is thus learned on the job.

The most important freedom for the young scientist is freedom to travel so that neither political nor financial considerations prevent him from working in the scientific laboratory of his choice. I have suggested that international laboratories in Europe and America would offer especially favorable conditions to students from remote countries. Of special importance in maintaining a high level in science are meetings of the scientific societies and of specialist symposia, provided that these are open to younger scientists. Freedom of the young scientist to work in other countries is essential if his country of origin has no high level of performance in his field of interest. It is desirable for a young American scientist to have experience abroad, despite the preeminence of American science. By being a scientist one becomes a member of a supranational society.

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I have developed the theme of this article extensively in a recent book, *Facing Reality*.[6]

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DAVID Z. ROBINSON

Will the University Decline as the Center for Scientific Research?

SCIENCE DEPARTMENTS at universities seem caught in a profound malaise. Many faculty members have lost confidence that they will be able to produce either good science or good scientists in the future. Their reaction may be the inevitable hangover after the expansion binge of the 1960's, but it corresponds to quite sober forecasts regarding the need for new institutional arrangements for scientific training and research.

The need for large and costly research tools, funded by the Federal government, brings inevitable pressures for the development of central research organizations with their own staffs. The necessity for new forms of organization was first felt in the fields of physics and astronomy, as accelerators and telescopes became more expensive. But costly large scale research is increasing in importance in other fields as well. Some biological and medical research—for example, the "million mouse" experiment to test low level radiation—requires organization of a scale and nature that does not easily fit in the university setting. Even in the social sciences, large scale surveys, such as those that led to the Coleman report, cannot be managed by universities.

Most forecasters have predicted that as government laboratories and centralized research facilities grow, the power and influence of university science will diminish. National science policy and even the internal development of scientific fields will be determined, they say, by non-university scientists and by the government itself.

The purpose of this paper is to examine changes in research arrangements in the field of high energy physics and, briefly, in astronomy, particularly in terms of the role of university scientists. It is my contention that, although organizational arrangements have changed significantly, university scientists have remained in control. This is the result both of their adroit political maneuvering and of their well-established status and prestige. I forsee that university scientists will continue to make the most of their assets in order to affect strongly the nature and extent of institutional changes in the sciences. The report that university science is dying is greatly exaggerated.

The Impact of World War II

The end of World War II may have been the high-water mark for university scientists, particularly physical scientists. They had left their college labora-

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tories and gone off to win the war. They showed, quite dramatically, that basic scientists could make major contributions in applying science to the development of new weapons such as radar, atomic bombs, and the proximity fuse. (One scientist in trying to explain why radar was developed by university physicists rather than electrical engineers said, "We were light on our feet.") They had developed a whole new field of study called operations analysis, a field aimed directly at practical applications.

The public—or at least the government—concluded that basic science was the best training for all research and that universities were the best place to do basic science. Most of the scientists quit the government laboratories after the war and went back to their colleges. The best of them took time off to continue as consultants to the government on major issues—to the envy (or bemusement) of their humanist colleagues.*

During the fifties, despite the explosive growth of government and industrial research laboratories, much of the good science and many of the best scientists were still in the universities, and the government was continuing to increase its research support of them. The late 60's, however, brought increasing unease. The scientists who, in advisory roles, had shaped the basic science programs and the pattern of government support seemed to be declining in influence. Research budgets were stable in dollars and declining in terms of spending power. Jobs were not plentiful for newly graduating Ph.D.'s and graduate students were getting restless. The public's attention was wandering. The rising and falling fortunes of university science research over the years is illustrated by the course of high energy physics.

High Energy Physics

High energy physics deals with one of the ultimate questions of physics, the nature of matter. Particles accelerated to high energies are used as probes. The higher the energy, the smaller the structure that can be examined. (This is all explained by the Heisenberg Uncertainty Principle.) Investigators in the 20th century, by increasing the energy of the accelerated particles, moved from the study of the structure of the atom, to the study of the structure of the nucleus, to the study of the structure of the particles of the nucleus.

As time went on, the earlier inexpensive accelerators developed in universities became obsolete. The experimental physicists were able to invent ways of getting higher and higher energy and intensity, and the results of their experiments were sufficiently perplexing to call for greater and greater ingenuity and

* In his Phi Beta Kappa Poem at Harvard in 1946, W. H. Auden said of the University:

"Professors back from secret missions
Resume their proper eruditions,
Though some regret it;
They liked their dictaphones a lot,
They met some big wheels, and do not
Let you forget it."

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understanding on the part of the theoretical physicists. In the effort to get higher and higher energies to examine more and more particles, however, the costs of the accelerators—and of the experiments done on them—went up very rapidly.

Thus high energy physics in the 1930's involved a few scientists at universities building their own relatively modest accelerators for their own and their colleagues' research. Despite the relatively high costs, and the managerial requirements of constructing and maintaining even these rather straight-forward facilities, the image of the university physicist as the lone experimenter working in his laboratory pretty well met the test of reality.

During the late 1940's and the early 1950's a number of university scientists proposed having the government rather than the university build accelerators for university research use. (Up to that time, the costs had been borne by the universities themselves in part through ingenious and vigorous fund-raising such as that by E. O. Lawrence of the University of California.) The government agreed rather readily and funds for construction and operation of more than ten university accelerators came from the federal agencies interested in science, primarily the Atomic Energy Commission and the Office of Naval Research.

While the funding had changed, the pattern of individual university-based research was very similar to the prewar style. At the same time, however, the Atomic Energy Commission set up national laboratories. The AEC was primarily interested in building and operating nuclear reactors, but they also wanted to build large accelerators which could accelerate particles in the billion-volt range.

Despite the growing role of government, effective control of the direction of the science was still in the hands of the university scientists. Not all the proposed accelerators were built and not all research projects were funded, but when choices were made, they were made—on the whole—by university scientists acting in review and advisory capacities. Furthermore, if an accelerator proposal were accepted, the university's own research group would choose the site, assume responsibility for building the accelerator, and carry on the research program.

In some cases the accelerator laboratory was part of one university, in others it was run by a group of universities. In practice, however, particularly when the site of the laboratory was not on the campus of the home institution, the degree of control by the university administrators (as opposed to scientists) was small.

In the late 1950's and early 1960's it became clear to most of the people wishing to do research in high energy physics that, pleasant as it was to do your own research on your home territory, the major discoveries would probably be made on the newest and biggest machines. It also became clear that there would not be enough machines to go around. In the cyclotron design of E. O. Lawrence, the cost of the accelerator grew rapidly after the energy of the particles passed a certain point, thereby setting a *natural* limit to the size of the

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cyclotrons that were built. Thus, in time, many good investigators could get a cyclotron. When the linear accelerator and synchrotron were developed, on the other hand, the cost per electron volt became almost constant. You could have one 100 Gev* synchrotron for about the same price as two 50 Gev synchrotrons, and scientists seem to agree that the one larger machine was more useful.

It was therefore clear that there would not be machines for everyone. For one thing, the cost of possible new accelerators in the late 1950's was in the tens of millions of dollars; by the mid-1960's the cost was ten times as great. Accelerator designers who had built and run machines that cost $20-40 million were ready to try to build machines that would cost from $200 million to $1 billion! At this level of cost, decisions about building went to the highest levels in the executive branch of government. $100 million, after all, is the accuracy with which Federal budgets are described. Thus a president would have, say, a $1.8 billion deficit or a $1.9 billion deficit depending on whether or not he agreed to build an accelerator. Clearly this is the type of decision that presidents want to make personally.

A further reason for increasing concern by politicians was the belief that large expenditures for basic research at universities or elsewhere helped the areas where the laboratories were located not only directly but with a multiplicative effect. People pointed to the industrial research laboratories on the Stanford campus and on Route 128 around Boston and decided that industrial growth was due to the presence of federally sponsored research. Politicians who couldn't tell an electron from a proton scrambled to have accelerators built in their districts.

In the face of these forces, it would be naive to say that decisions on research investment could be made solely on the rational ground of what was good for the science. In order to do the best job possible for science, to satisfy competing geographic areas, and to give at least the appearance of fairness, a new mechanism was devised: the *advisory committee.*

The Era of the Advisory Committee

As politicians have known for some time, advisory committees perform very useful functions. First of all they delay decisions, without giving the appearance of delay. Second, they give the appearance of impartiality. Thus recommendations that suit the politician can be peddled as the recommendations of the best available experts. And recommendations that go against the politician's ideas can be ignored. (President Nixon carried this technique to a new plateau by repudiating potential recommendations of two Presidential Commissions *before* the recommendations were made.)

Nevertheless, in the area of science, advisory committees made up of scientists are perhaps the best way for the scientific establishment to influence de-

* The units of acceleration are Mev (for million, or 10^6 electron volts) and Gev (for giga, one thousand million or 10^9 electron volts). This number is a measure of the maximum energy of the accelerated particles.

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cision making. They are by their very nature made up almost exclusively of scientists with close university ties. Although there is some attempt to exclude people with direct conflicts of interest (it would be unseemly to put the designer of an accelerator on the committee charged with evaluating it), it is hard for committee members to avoid commitment to the science itself or to their own regions of the country. The committees do tend to be conservative in their nature and, to some extent, to compromise and refuse to make hard decisions unless pressed by politicians.

Although advisory committees had been used by agencies to help develop their programs, the first major use of an advisory committee at the Presidential level came in 1958 when a group at Stanford University proposed building a two-mile long accelerator that would cost over $100 million. The committee recommended construction, and President Eisenhower announced the approval of the executive branch. Partly because the announcement was made at a banquet, without consulting the Joint Committee on Atomic Energy, but mainly because the Joint Committee was annoyed that some of its other pet projects had been turned down by the administration, the recommendation was turned down by the Congress. In 1960, a second advisory committee and the support of the Kennedy administration helped get approval, finally, from the Congress.

The approval, however, did not mean that Stanford University had complete control of the accelerator project. Because it was so large, and because the machine would be unique, a separate organization was set up, the Stanford Linear Accelerator Center (SLAC). Although SLAC was under the legal supervision of Stanford, most of its scientists did not teach physics courses and had relatively little to do with the academic program at the university. Moreover, the management of the laboratory involved the use of committees from outside to help schedule experiments and determine policy. Thus, although final authority was left with the director, in fact outside users had unusual influence. (A similar situation existed in the scheduling of experiments on the Brookhaven 30 Gev proton accelerator.)

Finally, there was a ceiling put on the number of permanent SLAC physicists. It was obvious that the people on the site had an advantage in proposing research projects because of their proximity and their day-to-day knowledge of what was happening on the machine. Thus by artificially restricting the size of the on-site group, the government made the requirement, independent of scientific considerations, that outside users would run a majority of the experiments at SLAC. Despite the university setting (which added prestige to the non-teaching SLAC scientists), SLAC was clearly a *national* facility.

These concessions to outsiders were very much desired by the scientific community. While the ideals of science call for the selfless pursuit of truth, scientists are not exempt from competitiveness and desires for personal fame and prestige. Knowing that they could not have access to a fine machine like the SLAC accelerator on their own campuses, outside physicists wanted to make sure that they had a fair chance to get time to run their own experiments at Stanford.

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The institutionalization of the role of the user, a scientist who performs an experiment on an accelerator, often bringing equipment with him from his home laboratory, has changed the traditional relationship between home scientist and visitor. Users have rights. They are also supported by the Federal government, and while working with students from their home institution, they relate scientifically to the machine on which they do their work. Their scientific reputation, and that of their students, is dependent on the goodwill of the accelerator director. It is no wonder that the users demand representation, rather than request visiting privileges.

The development and construction of the Stanford accelerator was similar to that of previous accelerators: proposal by a group, acceptance (after political activity) by the government, and then construction by the proposing group. However, the operation of the accelerator involved the group with strong outside scheduling and policy advisory committees, and was a break from the pattern of the past.

The 200 Gev Accelerator

The power shifted further away from the scientific community in the negotiations that preceded the construction of the 200 Gev accelerator at Batavia, Illinois. The first steps were the same, but the political pressures increased, in part because of the scale of the operation, and in part because of the decision of Midwest scientists and university presidents to involve themselves in the political process directly and personally in an unsuccessful effort to promote an earlier proposal.

In 1953, a consortium of sixteen Midwest universities set up an accelerator design group in Madison, Wisconsin, called the Midwest Universities Research Association (MURA). The organization was put together in part because the nearest national laboratory, the Argonne National Laboratory, was concentrating on reactors. They also shared some resentment of the University of Chicago, which managed the Argonne laboratory.

This group, after having been bypassed twice, in favor of accelerators at Argonne and at Stanford, had developed by 1962 a proposal for a 12.5 Gev accelerator of very high intensity,* to cost about $125 million. At the same time, the Lawrence Radiation Laboratory at Berkeley was developing, about a year behind MURA, a proposal for the construction of a machine of about 150 Gev energy but of lower intensity, that would cost about $150 million.

There were thus two competing proposals. The response to these proposals by the executive branch was to appoint a new advisory committee (known as the Ramsey Panel, because of its Chairman Norman F. Ramsey of Harvard) with some geographical "balance" and significant representation outside the field of high energy physics. Although the committee endorsed the construc-

* The intensity, or number of particles accelerated per second, is an important design feature.

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tion of *both* the MURA and the Berkeley accelerators, it gave clear priority to the "construction by the Lawrence Radiation Laboratory of a high energy proton accelerator of approximately 200 Gev energy."

After receiving the panel's endorsement but not priority rating, the Midwest scientists and universities enlisted their congressmen and senators in support of their proposal. In the end, despite a campaign which included sending a special delegation of university presidents and congressmen to visit President Johnson, the decision was made by the President personally not to build the MURA accelerator.

The decision to turn down the MURA accelerator, however, led to a new decision by the AEC. Instead of automatically building at Berkeley the proposed machine, now increased to 200 Gev and later to be approved by the President, the AEC threw the building site open to competition. It also set up a new national consortium of universities (Universities Research Association, or URA) to run the program. The National Academy of Science conducted the site survey. Over forty states put in proposals, many of them filled with elaborate data. Colorado had a beautiful picture of the Rocky Mountains on its cover. Teams of scientists visited each of the proposed sites, where they were welcomed by governors and congressmen. After over a year of work the Academy Committee named six preferred sites. The one picked by President Johnson was at Batavia, Illinois.

The AEC offered the Berkeley group, who had written the original proposal for the 150 Gev machine, the opportunity to move to Batavia and build the accelerator. When they refused, the AEC picked Professor Robert Wilson of Cornell. For the first time, control of site selection and construction had passed out of the hands of the scientists who proposed building the accelerator and into those of a Federal agency.

The organizational structure controlling the Batavia accelerator is now as follows. URA holds the contract from the AEC, and Wilson is the director of the laboratory. He is advised by a committee of physicists which passes on the proposals for use of the laboratory. This committee, to which Wilson must answer, is dominated by university scientists and made up entirely of people whose first responsibility is to their own institutions.

Given the size of the investment (approximately $250 million), the fact that it was the only large accelerator to be built in the country for many years, and the fact that politicians had become heavily involved, it is perhaps not surprising that the university scientists responsible for the original proposal should have lost out, or that the group finally responsible for operating the accelerator would have only limited control.

Much time during the Ramsey Panel discussions was spent on the question of how to manage the laboratory so that users could get significant amounts of time on the machines. Once again the panel recommended limiting the size of the on-site group to require the use of outsiders.

At the time of the Ramsey Panel deliberations, one of the major ways for a university to do high energy physics research was to set up a bubble chamber

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group. This group would do experiments using the bubble chamber at the accelerator site, but the substantial work of analyzing results would be done at the home laboratory. Faculty and students would thus spend their time, except during an actual "run," on the home campus.

Recent developments in experimental research, however, particularly in the use of counters and spark chambers, has led to needs for rather more time setting up the experiment on the site, and much less time analyzing it at home. This has made it harder than ever for faculty and students to interact with colleagues on their home campuses.

This change increases the pressure for the university to decline and the on-site group to increase in importance. This might indeed be the best balance from the narrow viewpoint of immediate scientific advance. Enormous amounts of time must now be spent coordinating the work of all the users. Thus, an observer might conclude that the demands of science would strengthen the central laboratory and weaken the university professors.

Yet, at the present time, the university users still reign supreme. The reasons are probably the following:

1. The universities as a group manage the program and have more influence in Congress.
2. The trustees have made the decision to keep the number of physics groups on-site small, and relatively weak.

Thus, apparent efficiency has lost out to political forces in the physics community. In this field at least, despite the centralization of the best equipment, there is almost no drift away from the university as the center of power.

The Situation in Europe in High Energy Physics

To those of us abashed at the political infighting involved in our own regional competition for accelerator sites, the situation in Europe in the early 1960's looked idyllic. There, the decision to build a large accelerator at CERN (near Geneva on the Swiss side of the border with France) with multinational funding and operation seemed ideal. Everyone seemed to be working for the good of science. CERN had long-term planning and multiple year funding. It had a good chance of cooperating with the Soviet Union.

Discussions between Norman Ramsey and the Europeans convinced the CERN people that they should build a similar high energy 300 Gev accelerator, to be finished shortly after the U.S. 200 Gev machine.

The proposal went to the scientific communities and governments in the countries that were supporting CERN. France seemed enthusiastic. Britain asked its Advisory Committee on Scientific Policy (ACSP) for a recommendation. The Committee, composed largely of chemists, said that the machine sounded good but that the British share of funds should not be taken away from support of science within Britain. (Some physicists called the ACSP the "Association of Chemists for the Suppression of Physicists.")

The problem of choosing a site was even more complicated than it was in

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the U.S. Some countries seemed willing to contribute only if they could choose the site. (The Germans thought the best site was in Germany. The English thought it was in England. The French thought they knew the best place too.) The final decision was to build the machine at CERN, probably on the French side of the border.

Thus Europe seemed to be going the route of centralization. The work at CERN in the 1960's was as good as that in the United States. Nevertheless, it seems today as though there is some decentralization taking place. National machines and university groups are getting stronger. Thus, although Europe has a history of doing much basic science in institutes outside the university, university dominance on the American pattern may be emerging.

The Field of Astronomy

Although high energy physics might seem to be unique, the situation in that field is closely paralleled by that in astronomy. Before the 1960's, much work was done at individual, often privately funded, observatories. The advent of the space age, of radio astronomy, and above all of recent discoveries such as pulsars and quasars, seem to call for renewed effort with large facilities.

There are national optical and radio observatories in the United States, and the space program is centrally run. The recent Greenstein Committee report to the National Academy calls for expensive new steps to enable universities to exploit these facilities.

Richard Metz, in *Science* magazine, commenting on the report, says: ". . . Fears that the national observatories would dominate astronomy, to the point of squeezing out university-based observatories, surfaced repeatedly during the meetings of the survey committee."

The astronomer's fears, which may or may not materialize, seem to be of two kinds. Some feel that the science will not advance well (if, for example, sixty percent of the time at the National Observatory at Kitt Peak is reserved for outside users, then there may be no opportunities for important observations that require large blocks of time). On the other hand, individual astronomers are concerned that scientists with access to better facilities may get all the credit. If astronomy follows in the footsteps of high energy physics, the second fear will overcome the first.

Conclusion

Those who felt that the advent of expensive research tools would move the center of gravity of basic research from the universities to federally-funded central laboratories have been proved wrong so far.

Indeed the central laboratories, although they are national facilities, are so organized as to consciously support university research groups. This support is secured by university domination of laboratory boards of trustees, by the

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predominance of outside users on advisory committees, and by the requirement that a large fraction of time be reserved for outside users.

These restrictions were not obviously determined by the good of the science, but they find favor with the scientific community because its most prestigious members are academically based, and with the political community because universities are a larger constituency than national laboratories.

In fighting for their own self-interest, however, university scientists may in fact be optimizing the long run health of basic research. A central laboratory can ossify, and it takes unusual talent to determine what kind of staff would best serve the institution on a long term basis. The fact that Europeans seem to be retreating from the centralized pattern to something approaching the American style seems to endorse this view.

If there is to be a change in the predominance of university scientists in American science, it must come through a qualitative change in the nature of research or in the nature of the university faculty. So far, in the case of physics or astronomy, the nature of research is still the same as it has been. Only the scale is different.

Where new kinds of research are needed, as in multidisciplinary fields, more striking changes might be forthcoming. Cooperation on campus is difficult to manage.

In addition, there are indications that faculty size may be stabilized or even reduced at the end of the 1970's and in the 1980's. Such a situation is unprecedented in American history. If the best young scientists do not find university positions and go to other types of institutions, such as federal laboratories or new types of research institutions, they may wrest control from the university scientists because of the scientific merit of their work.

The experience from high-energy physics, however, seems to show that although new groups come in to play new parts, the prestige and flexibility of the university scientists have kept them preeminent. They are indeed "light on their feet."

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J. B. ADAMS

Some Problems of a Big Science

THE TERM "big science" was used by Alvin Weinberg to describe scientific research carried out in large central laboratories on a very extensive scale, and in his book *Reflections on Big Science* Weinberg includes nuclear particle physics among the big sciences. Central laboratories for nuclear particle physics exist to provide experimental facilities for research physicists who belong to the universities and consequently carry out normal academic duties as well as research. The central laboratories were set up when the size of the equipment needed for the research became too big to be contained in a single university or even in a single country. This arrangement works extremely well and provides young university physicists with opportunities for carrying out research which their predecessors hardly dreamed of enjoying, and with personal contacts with their colleagues all over the world which are more frequent than in many other sciences.

However, this method of carrying out fundamental research presents certain problems and, since it is possible that other academic researches may one day evolve in a similar way to nuclear particle physics, these problems may be of general interest.

Explaining the origin and nature of the problems in this research inevitably gives an unbalanced view of the field as a whole. It could easily be concluded by the casual reader that physicists involved in this research spend their days in neurotic contemplation of their doom. In fact, for the vast majority, these problems are but small clouds on a distant horizon, and only in times of financial crisis and moments of great decision do they preoccupy the whole community of research physicists. No doubt they should receive more attention but the life of an experimental research physicist in this field is full enough already and, like his colleagues in other academic researches, he finds that the challenge of the problem in hand, the excitement of new results, and his teaching duties usually occupy all his attention.

The problems facing nuclear particle physics research, which are discussed in this article, stem from the dependence of that research on very large experimental apparatus, particularly nuclear particle accelerators. This dependence is not the result of an arbitrary choice made in the past by research physicists but is imposed by the nature of the investigation. Molecules can be broken

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down into atoms with very little applied energy. Atoms can be broken down into nuclei and electrons, at energies of a few tens of electron volts. To break down nuclei into their component nucleons requires a few million electron volt energies. But to break down nucleons themselves and to investigate the structure of matter at this level requires nuclear particle acclerators giving particle beams of many tens of thousand million electron volts energy. Apart from the use of cosmic rays, which offer some possibilities for limited experimentation, this research can be carried out only by using particle accelerators. The most recent accelerators, of which there are only two under construction in the world, are designed to give proton energies of 400 thousand million electron volts, measure over two kilometers in diameter, and cost of the order of 1000 million Swiss francs each ($264,000,000).

Before discussing the problems which arise in a fundamental research field in these circumstances it is worth considering how it came about that nuclear particle physics arrived at a situation where it could not only consider asking for such giant machines for its research but is actually in the process of building them. To answer this question requires a brief account of the history of this subject which by any standards is extraordinary and possibly unique among the academic researches.

History

Nuclear particle physics research is the present frontier of an inquiry into the fundamental building blocks of matter which started with the ancient Greeks if not earlier. For many centuries, however, this inquiry mainly interested philosophers and it was not until the second half of the nineteenth century that the first series of building blocks, the elements, were systematized in a periodic table. Towards the end of that century the electron, the first of a series of more fundamental building blocks, was discovered, and most of the major discoveries in this field have been made in the twentieth century.

In 1900 Planck discovered the fundamental rule of the quantum of action and Einstein, five years later, developed the relativity theory. These formulations, together with Rutherford's discovery of the structure of the atom in 1911 and Bohr's interpretation and application of the quantum idea to the atom enabled the properties of the atomic world to be understood, at least in principle, and explained the scale, the energies, the molecules, the atoms, the chemistry and all that we see around us.

Quantum mechanics was worked out in the remarkably short period of two years, between 1926 and 1928, by Schrödinger, Heisenberg and Dirac, and by the end of the 1920's the synthesis of physical phenomena in terms of electromagnetism and gravity, using the three building blocks, protons, electrons and photons, was so successful that to some physicists it appeared that all there was left was a tidying-up operation to complete the picture. The inquiry seemed to be reaching a triumphant conclusion.

However, with the discovery in 1932 of the neutron and a new force of nat-

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ure, the nuclear force, the research went on to reveal a completely new world of phenomena so that, far from being closed, the subject has opened out amazingly and all attempts to close it since the 1930's have been in vain. Thus, nuclear physics can be said to have started in the 1930's and it is the events of the last forty years which constitute its history.

Up to the outbreak of the Second World War, the research continued in most universities on a scale and with a style which was similar to that of other academic scientific researches. The exception, which foreshadowed future developments, was the research at the Radiation Laboratory of the University of California at Berkeley, where E. O. Lawrence created a new style of laboratory with large apparatus designed by teams of engineering physicists. Just before the outbreak of the Second World War a very large cyclotron was under construction at the Radiation Laboratory, the scale and cost of which were well beyond the capacities of normal university departments.

During the Second World War an event took place which completely changed nuclear physics research and all those engaged in this hitherto peaceful academic inquiry. This event was the development of the nuclear fission bomb. After the war yet another application of the knowledge gained through nuclear physics research—the generation of electrical power using nuclear fission reactors—was pursued on a large scale; the nuclear *fusion* bomb was developed, and research was started into the possibility of harnessing nuclear fusion reactions to peaceful energy production. These applications, because of their importance to governments and their dramatic effects on the world at large, affected the fundamental research and researchers in many ways.

First, the research became a matter of public interest and government concern and ceased to be an academic inquiry of interest only to a few physicists in the universities. When government agencies were set up after the war to develop nuclear energy in both its military and civilian applications, their terms of reference included, or were interpreted to include, fundamental research in nuclear physics. The way in which these agencies carried out this task varied in different countries. In the United States the agency concerned gave contracts to the universities to carry out the research and even the applications. Thus the University of California not only continued with nuclear physics research at its Radiation Laboratory at Berkeley but also managed the weapons laboratories at Los Alamos and Livermore. In Britain, the Atomic Energy Authority set up its own establishments for the applications and carried out nuclear physics research in its laboratory at Harwell while the university departments concerned with nuclear physics were funded from another government source. This arrangement was later changed, however, so that nowadays this research is once again back in the universities and its funding comes from the same source as other scientific researches in the universities. In other European countries part of the research is now carried out in atomic energy laboratories and part in universities, with funds coming from different governmental sources.

Secondly, due to the scale of operations necessary during the war for devel-

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oping the applications of nuclear energy, many research physicists who took part in these activities and returned to fundamental research afterwards were well accustomed to large scale engineering and project management. Hence, what had seemed well outside the possibilities of university research before the war did not seem so afterwards. People began to plan nuclear particle accelerators of a size far greater than the experimental apparatus encountered in other fields of fundamental scientific research and they demonstrated that they could build these machines and manage their laboratories. Thus, the effect of the war-time and post-war applications was to advance the time when it was possible to build such accelerators. Had the applications never occurred they would probably still have been built sometime, as the pre-war experience at the Radiation Laboratory at Berkeley clearly foreshadowed.

Thirdly, the applications of nuclear energy, particularly after the war, demanded skilled scientific manpower and especially physicists trained in nuclear physics. The source of such scientific manpower was naturally the universities and hence those countries concerned with these applications expanded the relevant university departments accordingly. Since research and teaching are closely related in the universities, the research budgets expanded along with the requirements for manpower. Thus, not only was it possible to plan large-scale experimental apparatus for nuclear particle physics after the war, but the research budgets increased sufficiently to be able to afford them.

Lastly, due to the importance of the applications of nuclear physics, nuclear physicists became very important people in the eyes of governments and sometimes even in their own estimation. Considering the use that had been made of their discoveries, this was by no means surprising. Consequently, when governments set up scientific and technical advisory committees after the war they often asked senior nuclear physicists to be members and the impression grew up among other disciplines of science that this helped to make government funds available for nuclear physics research.

Harvey Brooks has likened the history of nuclear physics during and after the Second World War to the course of a comet around the sun. The research, originating in the universities, became involved in the fires of central government and now is swinging away again to cooler regions. But the comet emerging from this encounter was much changed.

The last phase in the short history of nuclear physics concerns nuclear particle physics. When accelerators grew too big to be comfortably accommodated by universities, first national laboratories were established and then international laboratories. Today, in America, there is only one very large proton accelerator, the 400 GeV machine at the National Accelerator Laboratory near Chicago. In Europe, countries joined together to set up a single international laboratory near Geneva, called CERN, to house their largest machines. In Russia a similar international laboratory was set up at Dubna, near Moscow, to serve the needs of research physicists in the Soviet Union and the eastern European states.

Thus nuclear particle physicists working at the frontier of this subject,

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must now travel from their universities to a national or international laboratory to carry out experiments, then return to their universities to teach and analyze their results. They work in large teams and are subject to constraints not encountered in other fields of research. The present generation of physicists was born after the Second World War and few of them have ever been associated with the applications of nuclear physics. The ranks of the older generation who experienced the stirring events of an earlier time are now thinning but problems remain.

Individual Identity

It is clear that a nuclear particle physicist teaching in a university and carrying out experiments in an international laboratory like CERN is working under very different conditions than say Rutherford at the Cavendish Laboratory in Cambridge University in the 1930's. In Rutherford's time a physicist could still conceive an experiment, build his equipment or get it built by his technician, and put it together in a few months if not weeks at a cost rarely exceeding a hundred pounds. The experiment could be carried out in a similar period of time and the results, if significant, could be published soon afterwards.

An experimentalist working in nuclear particle physics today is usually a member of a team of twenty or more physicists from several universities in different countries, and the team requires the supporting services of forty to sixty other people to carry out its experiment. The team uses apparatus, much of which it has not built, costing hundreds of millions of Swiss francs. An experiment takes three or four years to plan, set up, carry out, and analyze. Publication of the results is a lengthy business and the entire first page of the report is often taken up with the list of authors and the names of the universities to which they belong. Clearly, this style of research raises the problem of the place of the individual physicist in the research.

One usually thinks of creative work of any kind as an individual affair and of really original ideas as conceptions of single individuals. However, in scientific research, ideas often seem to arise at certain times—to be in the air so to speak—and often several individuals put forward the same idea almost simultaneously. Also the scientific method depends for its success on the adding of one piece of knowledge to others already existing to erect an edifice of learning. Each generation of scientists depends on the contributions of preceding generations. Thus the teamwork now essential in nuclear particle physics research is not necessarily detrimental to advancing the subject nor does it impede creative ideas. Indeed the interactions within a team often stimulate creative ideas. Thus it is only the position of the individual in a team and the identity of his contribution which is in question.

One way of examining this problem is to inquire whether gifted research physicists flourish under these conditions and whether young physicists are encouraged or discouraged from participating in fields of research which re-

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quire team work and the use of very large apparatus in big national and international laboratories.

As far as the present generation of nuclear particle physicists is concerned it seems that highly creative physicists have adapted remarkably well to the prevailing conditions of work; very few people have left this field explicitly because of the way experiments now have to be carried out. However, this may be because those now working in the field have helped to create the conditions under which it is carried out. Therefore the attitudes of young physicists entering the field are probably more enlightening. Unfortunately no opinion poll seems to have been taken on this question so one is reduced to speculating as to motivation, and to taking the total numbers entering the field each year as a guide to its attractiveness. As far as the number of young physicists taking up nuclear particle physics research is concerned, there seems to be no dramatic falling off in recent years, at least in Europe, but the number is not increasing much, if at all. As far as motivations are concerned, one would expect that those fields of research which involve teamwork, large apparatus and extensive organization would be less attractive to a young research scientist than research fields without these characteristics simply because, in the latter case, there is more chance for a young man to make a substantial individual contribution to knowledge and thus win recognition. However, the motivations of a young research scientist in his choice of subject seem to be far more complicated than this. Sometimes he has a passionate interest in a particular subject, often due to the inspiration of a teacher who is himself active in the research, but such factors as how the budgets for the research are shaping up for the future and how society, especially student society, views the research may have a far more dominant influence. Needless to say, unless a research subject is assured of a steady and adequate input of the best brains of the day it will inevitably languish in time.

Technology and Organization

One of the obvious features of modern nuclear particle physics is the massive technology and extensive organization involved, both of which affect the style of the research carried out. An experimental physicist at CERN, for example, is surrounded by very large and complicated apparatus and must cope with a vastly greater amount of technology than in the past. This unavoidable involvement in the technology of experimentation can distract his attention from the physics of the experiment which is, or should be, his focus. It is only too easy for him to mistake technological virtuosity for an ability to contribute to fundamental knowledge. Furthermore, he can be tempted to select his experiments, not just for their potential scientific merit, but for secondary reasons such as the skills and knowledge available in the team with which he works, or the availability of the required apparatus, or even the chances that the experiments will be given time on the giant accelerators used for all the research. No doubt such secondary reasons for carrying out an experiment have always had

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an influence, and still do, even in fields of research carried out in small university laboratories with modest equipment. However, when the apparatus is very large and expensive, when the laboratory serves all the research scientists of a whole continent, and when the organization is consequently elaborate and necessarily democratic, secondary reasons for choosing an experiment can more easily affect purely scientific judgment. This is not to say that nuclear particle physicists now choose their experiments for trivial reasons—they certainly do not—but only that secondary reasons for the choice can weigh more heavily on the minds of the experimenters than they do in other fields and must therefore be more strongly resisted.

One way of at least alleviating this problem and freeing the minds of research physicists of the burden of technology is to shift it onto other shoulders. To some extent this has already been done in the big laboratories. For example at CERN a group of physicists has emerged who design and build the big accelerators but who do not take part in experimental research. As time goes on this tendency will no doubt spread to other fields of activity in these laboratories. Already the big computers used for data analysis are managed by specialists, as are many of the big particle detectors.

The question arises whether this trend is good for the research, whether it produces the most significant results and the greatest advances in knowledge. Arguments can be put for and against. The main argument in its favor is that it frees experimental physicists from technology and enables them to concentrate on research. The main arguments against it are that the specialists who develop and build the equipment may become insensitive to the needs of research, and that the experimenters may gradually lose control over their apparatus and even their understanding of its capacity for development. Thus experimenters might in time come to depend on technological specialists for their equipment as some are said already to depend on theoreticians for their inspiration. The logical end of this evolution is that the experimenters themselves will become specialists in carrying out experiments and the only true physicists left will be the theoreticians, a development which will upset the balance between experiment and theory on which the progress of scientific research has so far depended. Needless to say, such gloomy forebodings only lurk in the backs of people's minds but their awareness of such dangers slows down the drift towards specialization in the big laboratories.

The massive experimental equipment and central laboratories required by nuclear particle physics in Western Europe are organized in the shape of a pyramid. The wide base of the pyramid is made up of all the university groups in the member states of CERN who carry out nuclear particle physics research. The middle level consists of the national laboratories in the member states which provide experimental facilities of an intermediate size. Many of these laboratories are equipped with medium-sized accelerators for accelerating different kinds of particles, and some provide a central location where scientists can build up large experiments prior to their use at CERN and where data analysis facilities, including big computers, are installed for analyzing the results of

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experiments carried out at CERN. At the peak of the pyramid is the CERN laboratory itself with the largest accelerators and the biggest experimental facilities. Thus, CERN, like the national laboratories, exists to provide experimental facilities for the university groups which form the wide base of the pyramid. In this regard it is worth noting that the number of university nuclear particle physicists depending on the CERN facilities is over 1500 while only about 100 experimental research physicists are on the staff of CERN.

The financing of this pyramidic structure is also divided. Both university groups and national laboratories are funded by national sources, while CERN is funded jointly by its member states. In Britain, for example, the university groups and the two national laboratories are funded through the Science Research Council which is dependent on the Department of Education and Science. Britain also contributes to CERN through the Science Research Council, members of which are on the governing board of CERN.

Perhaps the most remarkable feature of this organization is that it works so well. However, being rather complicated and depending as it does on many governments and national organizations, its functioning is subject to many factors external to the research itself, over which research physicists have only an indirect influence.

Selection of Experiments

In any research field there is always the problem of deciding which experiment to do next. In a university laboratory with its own apparatus and a small staff the problem is usually discussed in the laboratory and resolved according to custom and the dominant personalities. In any case, it is a local affair and, except for the problem of money which may have to be sought externally, the experiment concerns only the faculty staff. In a big national or international laboratory like CERN the decision making process is inevitably more complicated, and the question arises whether the selection process used adversely affects the quality of the research carried out.

Proposals for experiments to be carried out using the CERN experimental facilities arise from the numerous university research groups in Europe either as individual proposals or, more commonly, as team proposals where the members of the team come from several universities and from different countries. The inspiration for an experiment comes from an individual, from discussions within a team, from interactions between a team and theoreticians, from the many conferences and seminars held every year; and usually, in some degree, it comes from all these sources combined. The proposals are brought to CERN and presented at seminars attended by all interested physicists, after which they are examined by experiments committees. During this procedure a proposal may undergo considerable improvement and the composition of the original team may be changed. The experiment may even be abandoned altogether for scientific reasons. Next, the experiments committees put together a research program for the coming year or two which consists of the experiments

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which have survived the critical process. Finally, the CERN laboratory, its budgets and its facilities, together with the national and university laboratories concerned are scheduled around this program. This system works quite well, as the scientific results over the years show, but the question remains whether it enables the most important experiments from the scientific point of view to be done at the right time.

It is not hard to point to some biases in the system. For example, an experiment of high potential value but high risk of failure is probably less likely to survive all the public criticism and committee deliberation than a worthwhile experiment which will almost inevitably lead to a valuable result. Furthermore, a dynamic and articulate team leader is more likely to persuade his colleagues in discussion and the committees in their judgment than a shy, reticent leader who may nevertheless be proposing a better experiment. Of course, everyone is aware of these biases and aims to offset them but some less obvious ones may still remain in the system. For example, although no evidence exists that the present system has so far rejected a critical and important experiment, it may be that the very existence of selection committees and the way in which they work prevents some proposals from ever being presented. In other words, individuals and teams, knowing the tough criticism which their proposals will be subject to by the committees may simply not propose risky experiments. Also the possibility that an experiment accepted and carried out at CERN at such a high cost in manpower and money will in the end prove a failure must tend to favor "safer" experiments. Thus the system may very well bias its own input and it is no consolation to imagine that experiments rejected at birth in this way will be done elsewhere since there are only a few other major centers in the world with the highest energy facilities and they all use very similar selection systems for their experimental programs.

Naturally, modifications to the present system have been considered over the years as people have sought to remove possible biases. For example, it has often been suggested that decisions should be based more on the quality of people than on that of proposals. The present system does indeed do this to some extent since the judgment of the committees is partly based on the quality of the team proposing an experiment. But some people would go further than this and give a few particularly brilliant individuals almost dictatorial control over some fraction of the use of the experimental facilities, still leaving the bulk of their use to be determined by the democratic selection system now in operation. However, this proposal merely exchanges the problem of selecting experiments for the equally difficult problem of selecting individuals. Who selects the brilliant scientists and how?

This system for determining the research program in nuclear particle physics can be contrasted with the way a few scientists in a small laboratory working with their own equipment decide on their experimental program. In a small laboratory, one significant result every few years is sufficient to compensate for a few unsuccessful experiments and to maintain the good scientific reputation of those involved. An unsuccessful experiment at CERN or one

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giving publishable though only moderately interesting results but involving forty to sixty people and millions of Swiss francs cannot so easily be ignored, particularly not by other teams who naturally feel that they could have used the men, money and facilities more profitably on their experiments. Furthermore, whatever heart searchings go on in a small team in a university laboratory about their experiments are usually a private affair and only when the team publishes its results does the full weight of public scientific criticism come to bear on its work. In nuclear particle physics, however, public criticism arises even before an experiment is started. One may well wonder how many of the famous scientific experiments of the past would have been done if their authors had had to face such a critical appraisal before they were allowed to start.

Teaching and Research

It has long been maintained that fundamental research is an essential part of the higher education system; that the functions of a university are to preserve knowledge, disseminate this knowledge to students, and seek new knowledge. Nuclear particle physicists, like other academics, teach as well as carry out research. However, unlike most academics, nuclear particle physicists must now carry out their research in national and international laboratories far distant from their universities and often in another country. Admittedly, members of some other university faculties are faced with similar problems—historians, for example, who must look at original munuscripts not at their universities and geologists and geographers who must do field work—but in these cases, the extramural research can usually be arranged to fit in the long vacation periods of the universities. No such arrangement is possible in high energy physics since single experiments run for several years and the experimental team works in shifts, twenty-four hours a day, for months on end. There is therefore a serious difficulty in combining teaching and research in experimental nuclear particle physics to which no simple solution has yet been found. One possible solution is to increase the staff of physics departments so that part of the staff can be away from the university for long periods leaving others to carry out the teaching duties. By a system of rotation, individuals in time would fulfill both research and teaching functions. Whatever solution or solutions are finally adopted, there is general agreement that the aim is to avoid a separation between teaching and research in the universities.

Research and Governments

A characteristic of nuclear particle physics often remarked by those outside the field is the money it spends in carrying out its research. This characteristic has not escaped the notice of the governments which nowadays provide this money. One reason why research expenditures in nuclear particle physics are so highly visible is the concentration of experimental facilities in national and international centers whose budgets form single entities which must be ap-

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proved by governments year by year. Also, every five or ten years nuclear particle physicists must ask for a very large capital sum to build a new piece of apparatus and this must be the subject of a separate governmental or intergovernmental agreement. In Europe, in recent years, a proposed new European nuclear particle accelerator to give 300 GeV energy protons was a much debated issue. The decision to build this was made only after more than seven years of intense discussion involving not only the whole of the nuclear particle physics community in Europe but also the governments of eleven member states of CERN and their national scientific research agencies. In the end it involved a reshaping of the pyramidic structure for this research in Europe, the effects of which have still to be fully realized.

In other fields of scientific research such problems are much less intense. For example, in biological and medical research, where total expenditures are comparable with those in nuclear particle physics, the few national laboratories which exist are small in size and run on relatively modest budgets, and international laboratories are only now being considered, for example, for molecular biology in Europe. Furthermore, the size of individual pieces of experimental apparatus is also relatively small and the decision to build them does not usually involve national or international decisions. The research is largely carried out in departments of all the universities in Europe, each of which manages its own modest budget and its own apparatus. Some other fields, however, such as optical and radio astronomy, are beginning to encounter problems similar to those of nuclear particle physics as telescopes reach sizes and costs comparable with accelerating machines. Thus it is nuclear particle physicists who first encountered the problem of justifying their research publicly, a laborious business which has by no means reached a satisfactory conclusion.

In a short article of this kind it is hardly possible to expand on the allocation of research funds to the various disciplines of science and the establishment of an overall national scientific research budget. The benefits which a country expects to obtain from its investment in fundamental scientific research are by no means clear and vary from country to country and from time to time. Nevertheless, two benefits seems more durable than others, namely, the benefit of carrying out fundamental scientific research in the universities to the quality of the sciences graduates they produce each year, and the benefit to society of the occasional practical applications which arise from fundamental research. Nuclear particle physics research, like other university researches, has contributed to both benefits in the past; certainly its practical applications have been remarkable. It may well be that other applications will arise from research in the future but these are unknown at present and can hardly be used to justify current investments. If it is agreed that fundamental scientific research is an essential part of university education, then nuclear particle physics research can claim to be an essential part of that fundamental research and some would claim that it is more fundamental than many other parts since it deals with the structure of matter itself. If, in addition, it is agreed that this research requires large pieces of experimental apparatus in order to advance, then, having cut

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the cost of this apparatus as much as possible by concentrating the largest facilities first in national laboratories and then in international laboratories, the only alternative to providing the apparatus is to abandon this field of research. There is, however, still one variable left and that is the rate at which this research should go ahead. The essence of fundamental research is that it is carried out with no practical aim in mind. It is simply a search for knowledge. Hence it is not tied to any external time scales but proceeds at its own pace. What then determines this pace, apart, of course, from the natural desire of research scientists to get on with their work?

In nuclear particle physics two factors may be mentioned; the first is international comparison and the second is the time needed to carry out experiments. Difficulties arise and have arisen in the past, when groups of research scientists find themselves so underequipped with experimental facilities in comparison to other groups that it is impossible for them to make significant contributions to the research. This situation arose in Europe in the early 1950's in nuclear particle physics and led to the foundation of CERN to provide experimental facilities for European physicists comparable to those available in America and Russia. Hence a time scale arises which depends essentially on the average pace of the research in those countries of the world in which it is being carried out. Although it is clearly impossible to achieve comparability at all times there are limits to how far the experimental facilities of a group can fall behind beyond which its further investment in research becomes questionable. No doubt a similar argument can be made for all scientific research work. However, the time it now takes to do an experiment is a problem specific to nuclear particle physics. Many years ago, an experiment in nuclear particle physics took less than a year to conceive, plan, construct, carry out and publish. Nowadays, typical experiments take three or four years and the signs are that they will take longer in the future. Hence an experimental physicist can expect to take part in only a few major experiments during his working lifetime and not all of these will turn out to be crucial experiments. This long term nature of research in nuclear particle physics, a problem not unknown in other fields of research (e.g. forestry), contributes to the urgency of its demands for up-to-date facilities for experimentation.

However, whatever the internal pressures arising within the research of a given group, it must be admitted that they are relative to what is happening generally in the world in a particular research subject. It is difficult in fundamental research to establish external criteria to determine the rate at which research should proceed and, when money is in short supply either for fundamental research in general or for a particular branch of it, this rate must be reviewed. It may well be that there is some lower limit beyond which research is hardly worth continuing, a point of diminishing attraction, perhaps, at which young research scientists might feel that they could not expect to make significant contributions during their working lifetimes and would consequently look elsewhere for research subjects.

In discussing such matters with governments and persons not familiar with

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nuclear particle physics research, the problem always arises of explaining the progress and achievements of the research itself. Any group of people which spends large sums of money supplied by governments from taxation has an obligation to explain what it is doing with them. Apparently not all research scientists recognize this obligation, but in nuclear particle physics it cannot be ignored. To recognize an obligation, however, is one thing; to discharge it successfully is quite another. Even a nuclear particle physicist most willing and anxious to explain his research to governments and to the public runs into major communication problems. There was a time in this research, when the field was first being explored and when new particles were being discovered every few months, when explaining research progress was not such a difficult matter. In recent years other branches of research have enjoyed similar happy periods, such as molecular biology with the RNA molecule and radio-astronomy with quasars and pulsars. However, the initial discovery of the basic entities and structures is usually followed by a long period of detailed experimentation and theoretical speculation which is far more difficult to popularize. Nuclear particle physics is now deep in one such period, very exciting and very rewarding to those working in the field but extremely difficult to explain to anyone outside, even to another scientist. No doubt when, as result of all the present research, a new synthesis appears, perhaps a new underlying structure which explains all the phenomena presently being researched, or a new theory which accounts for the nuclear force, it will once again be possible for gifted expositors to popularize the progress of this research. Meanwhile the problem remains of explaining what is now happening to those who are paying for it and it is quite evident that much more work and imagination needs to be devoted to this problem.

Conclusions

Most, if not all, of the problems which have been mentioned here either originate in the need of this research for very large pieces of experimental apparatus or are problems, latent in all branches of fundamental research, which have been greatly amplified by this need. Thus similar problems may be expected in any research subject which evolves in such a way as to require very large equipment and central laboratories which must necessarily serve many universities. For example, they may emerge in radio-astronomy when telescopes reach such a size that single countries can afford only one or later on when countries themselves are encouraged to combine together to afford an even bigger instrument. In optical astronomy it has already happened, and the European Southern Observatory is now under construction in Chile. Even in such a new field as molecular biology there is already a European organization which proposes to set up a single European laboratory.

If this stage is reached in a research field the subject becomes highly visible and cannot escape public concern and governmental scrutiny. Consequently, all kinds of external pressures are brought to bear on it which have no direct

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relationship with the fundamental research but, nevertheless, strongly influence its future. It therefore becomes a very vulnerable science. Flattering though it may be to some individuals to find themselves in the public arena, most research scientists are not well equipped to cope with the resulting problems and deplore the time and effort which must be devoted to them at the expense of research.

In a sense a research subject can be considered unfortunate to have reached this position and were there any way of continuing with nuclear particle physics research without using giant accelerators in national and international laboratories, many research physicists would prefer that way. Therefore, perhaps the most useful advice that nuclear particle physicists can give to their colleagues in other branches of academic research is to avoid, if they can, allowing their research to become dependent on very large and very costly experimental apparatus.

Finally it must be admitted that the need for giant accelerators and international laboratories has its compensations. For example, CERN has pioneered scientific collaboration not only in Europe but also with the Soviet Union and many other countries, and the successful experience of CERN has been used by governments to launch other collaborations outside the field of scientific research. Also, it is often remarked that a laboratory like CERN, where most nuclear particle physicists from Europe as well as many of their colleagues from America, Russia and other countries, spend some part of their time each year, provides an extremely stimulating and exciting center not only for carrying out experiments but also for exchanging ideas and finding out what is happening in this field all over the world. Thus the compensations, both for physicists and for governments, finally far outweigh the difficulties and the research is going ahead more vigorously than ever. Indeed, there are not many physicists working in this field who would admit that there are any problems at all.

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HARVEY BROOKS

Knowledge and Action: The Dilemma of Science Policy in the '70's

THERE ARE BASICALLY just two central questions regarding the organization and administration of science. These questions present themselves at many different levels of aggregate scientific activity, from the individual laboratory or university department, to multinational research institutes and collaborative programs, including research and development programs which are nationally funded.

The first question is how to organize, staff, and direct the search for knowledge so as to obtain the greatest rate of scientific progress for a given investment of human and material resources. The second question is how to couple the existing body of knowledge, as well as the search for new knowledge, to existing needs for policy or action, including those felt by education and technological innovation.

By scientific progress I mean not the mere accumulation of data and information, but rather the advancement of our codified understanding of the natural universe and of human behavior, social and individual. Such progress also embraces the conceptualization of knowledge about human artifacts, what Herbert Simon[1] terms the "science of the artificial," which is assuming greater relative importance as the conditions of life are increasingly determined by the man-made rather than the natural environment.

In practice, there are no simple objective measures of scientific progress external to the social processes of the scientific community which produces it. Thus, to evaluate scientific progress we are compelled to rely on a consensus of the scientific communities in each field. We are subject, of course, to uncertainties about which groups are competent to judge progress, and to the fallible fashions of any guild. Nevertheless, the highly structured system of mutual criticism; refereed publications; peer group evaluation of research projects; and personal recognition through prizes, fellowships, and academic appointments constitutes a kind of intellectual marketplace in which scientific contributions are valued in a more or less impersonal way. Available studies of this market, largely confined to the subject of physics, suggest that it is indeed reasonably objective in its operation.[2]

Modern nations devote substantial resources to the cultivation of science, not so much because they judge it worthwhile in itself as because they expect

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that scientific advance will foster some other social or national benefit. They assume that if scientists are encouraged, or at least permitted, to pursue their intellectual curiosity wherever it may lead, society will reap benefits in the most efficient and economical way. The autonomy of science is often defended by the scientific community in ideological terms, but viewed by others as a form of scientific arrogance which asserts that the scientist's own frame of reference is the only one appropriate for evaluating the output of science. What I am asserting is that the standards of the scientific community are indeed the proper measure of scientific progress, but not necessarily of the contribution of scientific progress to social progress. The latter must also be judged in other terms by other constituencies. I would maintain, however, that the most efficient scientific progress tends to make the greatest scientific contribution to society.

The second question, how to couple scientific knowledge and research to needs is subtler and harder to define. The potential social benefits of scientific progress cannot be realized unless the knowledge is either taken up by government decision-making bodies and industrial innovators, or contributes directly to educating the public and the next generation. Furthermore, knowledge by itself is always ambivalent; it has potential for evil as well as good. It can be used for purposes which many consider socially harmful, such as war, or it can put new tools into the hands of misguided or oppressive, but powerful men. More subtly and frequently, it is used with the best intentions for what are thought to be beneficial purposes, only to have it discovered afterwards that harmful secondary consequences are occurring. Frequently such secondary consequences increase faster than in proportion to the primary benefits as a technology becomes more widely diffused and used. It is these "non-linear" effects of technology which have often led to public reaction against technology, and even science, for the primary benefits are often forgotten in the light of the suddenness of the discovery of non-linear side-effects.

There are, in turn, two questions with regard to the utilization of scientific knowledge. The first is how to use knowledge which has been generated primarily for its own sake—fundamental research. How is this knowledge to be condensed, summarized, repackaged, and interpreted for use, and how is it to be communicated to those responsible for action—the decision-makers, the technological innovators, the service professionals, and the students who will fill all these roles in the future? The second question is how the needs of government policy, industrial technology, or professional service can and should influence the process of discovery itself. This is where the question arises of how human values and scientific knowledge interact with each other.

During most of the 1950's and 1960's discussions of national science policies and of research administration tended to focus on the first of these major questions, how best to promote progress in science itself, and in technological innovation generated directly by scientific research rather than by a previously perceived social need. Today the focus has shifted rather rapidly towards the second of the questions. Most contemporary criticism of scientific institutions is

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directed not at their effectiveness in advancing knowledge, which is almost taken for granted, but at their capacity to couple knowledge to action, and to the corollary question of what relevance the knowledge they generate has to the multiplicity of newly identified problems involved in economic and social progress, not only for national societies, but for humanity as a whole. Increasingly, the assumption that maximizing the rate of scientific progress as defined by scientists will in the long run automatically produce the insights most relevant to the solution of human problems is challenged, and attention focuses on the interfaces between scientists and their institutions on the one hand, and the rest of the social and political structure on the other. The movement of knowledge into decision and action, and the relationship between "objective" knowledge and the value structures of society has come to be a central concern.

This new climate of thinking, though most prevalent in the United States, is increasingly evident in Western Europe and Japan, and is beginning to be widespread in the Soviet Union.

Science and Political Decisions

In the past, science has been supported primarily because of the power it gives us to manipulate nature for human purposes. In the last few years, however, we have become more interested in another value of science, its capacity to provide the means for disciplining and channeling a technology which threatens to get out of control, or at least is so perceived by a growing public. Thus, in addition to providing the basis for technological innovation, science is becoming the basis of a new technological conscience which searches out the potential consequences of technology and questions its uncritical application.

In the U.S. this new function of science has projected scientists and scientific institutions into an unaccustomed political role, thereby generating a new set of public issues concerning the proper relationship of scientists to political decisions, particularly, but not exclusively, in the environmental field. Increasingly, scientists have been in demand as witnesses at hearings before congressional committees, administrative agencies, and even courts.

Both because of our general affluence and because of the absence of other forms of legitimation for social policy, there is probably a greater proliferation of such new social demands on science and scientists in the United States than elsewhere, but the same forces are present everywhere in advanced societies. The OECD report, *Science, Growth, and Society,* examined the new relationship between science and society; its enthusiastic reception by the OECD Science Ministers is indicative of its resonance for a broad spectrum of elite opinion in Europe and Japan.[3]

Historically both science and technological innovation have received much of their impetus from war or from political competition between nation-states. In most industrialized countries the largest fraction of technological effort goes

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into projects for national defense, space, and nuclear energy and a large fraction also goes into supporting national competitiveness in international trade. It is estimated that 25% of the world's scientists and engineers are engaged in military research and development, and the great majority of the rest are engaged in industrial research. In these areas a fairly comprehensive system of innovation has evolved, so that knowledge does move into action, and work in research and development is coupled to end products—though perhaps least successfully when governments have subsidized R. and D. directed at products and services for the private market which, outside the socialist countries, they have done most extensively in Europe and the United Kingdom.

Today we are witnessing a broadly based world-wide movement, both within and outside the technical community, to utilize science and technology for less nationalistic purposes: for the alleviation of poverty on a global basis, for better management and protection of the world's resources with minimum detriment to economic development, for the achievement of greater social justice, for the more reasonable allocation of the resources of the biosphere toward sustaining a decent human life for all people. In these areas, however, we are faced with a massive deficiency of experience in coupling knowledge to action. We scarcely know how to translate what we know into tangible benefits or wise social restraints, and conventional wisdom, good intentions, and moral fervor are hardly adequate. The very successes of technology in some areas has generated expectations which make its inadequacies in others seem the more glaring and inexcusable. The "if we can land a man on the moon" syndrome has transformed what we expect from technology, but, in fact, American successes in space carry relatively few lessons applicable to problems, like virtually all of those mentioned above, where conflicting interests and values play a large part.

Intrinsic vs. Extrinsic Strategy of Science

There are two limitations on the coupling of science to its social utilization. On the one hand, knowledge is seldom used unless an individual or group recognizes a need, translates it into meaningful and manageable scientific questions, and then acts as an energetic advocate of concrete action. A "manageable scientific question" is one which is answerable either out of the existing body of scientific knowledge or through a search for new knowledge guided by existing understandings and involving only limited extensions of the current conceptual structure of science. Pure research, isolated from those who understand it sufficiently to know how to use it, cannot provide inspiration for innovation or better decisions. Such research may be very valuable, but it requires an incubation period before it can be appreciated in relation to its possible applications.

On the other hand, attempts to channel the search for knowledge narrowly on the basis of imagined social needs or other primarily non-scientific objectives, usually result in frustration and expensive wasted effort. Such targeted

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research requires a critical mass of pre-existent basic knowledge to be carried out efficiently. Until this critical mass is accumulated, the strategy of research is better governed by the internal logic of its subject than by our limited vision of what is likely to be relevant to a particular practical problem or impending policy decision. The questions we begin by asking are not those we end by answering, and an insight that initially appears to have little relevance may rather suddenly turn out to be the key to further progress towards a practical goal.

The best scientific strategy generally lies somewhere between isolation from practical problems and narrow channeling on the basis of practical needs, but to locate the balance point in any particular case requires the most experienced scientific and general judgment. There is no simple formula for resolving this dilemma, but certainly it is folly to drop all basic work when a pathway to a practical solution appears to be in sight. Studies of successful development programs show that important ingredients in a final technical solution frequently do not appear until the design of the end product is already supposedly frozen.[4] As a method to control fertility in poor countries, the IUD was so widely hailed a few years ago that some proponents were even recommending that basic studies of human reproduction be abandoned in favor of all-out action programs. Yet today there is such widespread disillusion with the IUD in parts of the underdeveloped world, that they are accusing the developed countries of having foisted an impractical gimmick on them.

Centralization vs. Pluralism in Research

This issue occurs in two quite different contexts, first, in the organization and budgeting of science at the national level, in what is sometimes called "policy for science," and second, in the organization of efforts to achieve a given scientific objective. Is it better to gather together all the strands of research which might contribute to solving a given problem into a critical mass so that research can be channeled according to a grand strategy determined either centrally or by the consensus of a limited number of scientists interacting on a day-to-day basis? Or is it better to encourage a multiplicity of separate, even fragmented, approaches in many locations, with each scientist pursuing his own path, subject only to the indirect coordination provided by public scientific literature and by the informal communication networks which grow and dissolve within the scientific community?

At the level of national organization the question is whether all government scientific activities should be administered by one department, with its own organization and fiscal appropriation, or whether technical activities should be administered and budgeted as part of the total activity or mission of the specific department to which they relate. The latter departmental organization encourages the coupling of research to end uses, but at the price of autonomy and integrity in scientific planning.

In the United States, government science administration has a strongly

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pluralistic tradition, in which each government mission is responsible for its own research and development activities and budgets them in competition with its other operations. This provides, in theory at least, an orderly flow of ideas from basic research to the operational needs of the agency. Thus the Department of Agriculture is responsible both for agricultural research and for price supports. The Department of the Interior is responsible for geological research and for the management of natural resources. The Department of HEW is responsible for biomedical research and for the operation and financing of Medicare and Medicaid. The Defense Department is responsible for basic research relevant to military operations and also for operating the military services and procuring matériel.

In the U.S., government support of science evolved piecemeal in response to a succession of opportunities and needs arising in separate historical epochs. Only after World War II did the federal government begin to consider the support of science on its own terms a legitimate responsibility of government, and to contract to an appreciable extent for research to be performed outside government laboratories.[5] A national commitment to the support of basic research was clearly institutionalized only in 1950 when the National Science Foundation was created. Even as late as 1972, however, the research budget of NSF accounted for only 2.5% of total federal R. and D. expenditures, or 5.2% of all non-military ones. The NSF supported less than 10% of basic research and less than 20% of all federally supported research in universities. All other federal R. and D. expenditures were budgeted and administered by the departments responsible for functions defined in social rather than scientific terms such as defense, health, agriculture, environmental services, housing, transportation, and social welfare. In this scheme the large expenditures of the Space Agency (NASA) and the Atomic Energy Commission (AEC) are something of a hybrid, for neither space nor atomic energy is a social function in the normal sense. Rather, each represents a defined range of technology supporting a variety of social and political objectives.

In contrast, the Department of Education and Science (DES) in the United Kingdom, accounted in 1972 for 28.4% of all non-military R. and D. expenditures, almost six times as large a fraction as the 5.2% accounted for by the NSF, its nearest U.S. analogue. Most other industrial nations are closer to the British than the U.S. pattern.

There is, however, some trend towards convergence of the two patterns of R. and D. support. In the U.S. the fraction of government funds for R. and D. allocated by the NSF has steadily grown: for academic research, for example, it has grown from 15% to almost 20%. Furthermore, as its resources have grown, the NSF has gradually expanded its self-defined mission beyond that of supporting basic research in academic institutions. Although the legislation authorizing the NSF never prohibited it from engaging in applied research, a 1968 revision gave it an explicit charter to do so. During the past three years it has inaugurated a new program, Research Applied to National Needs (RANN), which partially implements this legislation. Although RANN includes both

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basic and applied research, it is problem-oriented in the sense that it is organized according to socially rather than scientifically defined objectives. It now accounts for about 20% of NSF's research budget and for most of the recent growth in NSF program staff. Some of the NSF's experienced basic research program officers have been placed on the RANN program staff.

RANN has encouraged the academic scientific community to organize itself for systematic multidisciplinary attacks on problems of national energy policy, environmental management, urban planning, evaluation of large-scale social programs, and other national needs. It also deals with technological innovation; in addition, a new program, labeled "technological incentives," provides for joint industry-university teams to work on improving the productivity of technological innovation as well as of activities related to the public sector. Significantly, the success of the RANN program is to be evaluated in terms not only of the scientific quality of the work which it produces, but also of the effectiveness with which its results are coupled to policy and action, and hence of their acceptance by action-oriented agencies both in industry and in government.

It is not surprising that the RANN program has been controversial, especially within the academic community. Some fear it is eroding the standards of intellectual excellence previously upheld by the NSF as the main criteria for supporting research projects, and that second-rate research is being supported lavishly because it promises practical results, while first-rate pure science languishes on ever more constricted budgets. Others argue, however, that RANN is encouraging a long overdue reorientation and restructuring of the academic science community, breaking down often artificial and obsolete barriers between disciplinary departments. Still others, mostly from industry, consider RANN an expensive form of re-education for university scientists. They say that if RANN's purpose were really to solve pressing social problems, the projects should have been assigned to teams in industry or to non-profit research institutes more accustomed to operating in a multidisciplinary, problem-focused context. Advocates of RANN point to the large number of new proposals generated from academic groups as an indication that the program meets a large previously unsatisfied need for support, and demonstrates the latent desire of many first-rate academic scientists to become more involved in action-related research with a potential social impact. Most academic scientists look upon RANN as applied research, though the officers of NSF are quick to point out that its projects actually include a good deal of interesting, challenging, and high quality basic research in a non-traditional, mission-oriented context. Debates rage as to whether discipline-oriented universities can or should be expected to organize themselves to perform policy-oriented research. Can academics ever be sufficiently connected to the point of ultimate use to produce important effects on action, or will their efforts result only in published papers of, perhaps, impeccable scientific quality, but with no real customer, reports that will be widely quoted, discussed, and duly referenced in other similar reports, but then gradually forgotten without attracting the attention

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of the decision-makers or achieving real credibility among those who are in a position to act on them?

All the arguments for and against RANN have some merit, especially when applied in isolation to particular projects. The program goals are ambitious, perhaps unrealistically so in relation to the resources and personnel available. In some cases, RANN has been oversold in terms of what practical results are realistically achievable within the time scale of a typical research project. On the other hand, the program has brought a fresh and innovative scientific and technical constituency into the attack on important national problems, much as a new constituency was brought to bear on military problems by OSRD early in World War II. The education and development of this constituency may well prove to be of much greater long range importance than the results of specific projects. RANN should also help to provide new perspectives and formulations of what our problems really are. In this sense it could be viewed as a mechanism for translating broadly defined social problems into specific manageable technical problems and simultaneously for developing a constituency capable of attacking them. However, it is probably too early to judge the success of the program, which will ultimately depend more on the degree to which its results and people are put to use by other agencies than on the specific output of individual projects. The greatest dangers are that the public may be led to expect that the RANN program, or any other NSF program, can by itself resolve the problems to which it is addressing itself, and that it may have insufficient political influence to acquire the other ingredients it needs to succeed.

In a different kind of evolution, NSF is also turning with increasing frequency to non-academic performers not only to do applied research, but to work in some of its basic programs as well, especially those in the environmental sciences that require large-scale collaborative effort. The RANN program uses non-profit research institutes and AEC national laboratories to a considerable extent, and the technological incentives program will use industry, although it gives preference to collaborative efforts of industry and university. Legislation now pending in the Senate would move NSF much further towards the support of industrial research and of technological programs to work on civilian problems such as those of housing and transportation. It would also provide for extensive retraining of space/defense-oriented scientists and engineers to participate in such civilian-oriented efforts. This augmenting of the power of NSF would be a clear move towards making it a department of science and technology, towards a centralization which, in my opinion, would be disastrous both to the health of basic research, and to the successful application of technology. Through RANN the NSF can and has developed a new innovative and catalyst role in launching attacks on social problems through research. However, responsibility for their implementation, including most development and virtually all demonstration and testing activity, should continue to lie with the agencies most familiar with the environmental, political, and

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economic constraints within which new technological solutions will have to be applied.

No major country has ever collected *all* of its governmental scientific and technological activities into a single administration. In virtually all countries defense research and development has been separated from the rest of the government science structure and attached directly to the defense establishment. Nuclear energy has almost invariably enjoyed some form of special status whereby it reported directly to the chief of state. The U.S.S.R. has probably gone further than any Western nation in budgeting defense and nuclear research separately from corresponding research in other fields, but in view of the highly centralized control of Soviet society in general, it is not clear how much this budgetary separation reflects a separation in the administration of R. and D. activity in these areas.

In many countries, such as the United Kingdom and West Germany, a single cabinet department administers both science and higher education. In the U.S. a congressional committee recently recommended the consolidation of federal science activities "which are carried out in universities or similar institutions, and which are closely related to the total operations of higher education and advanced study" into a single "National Institute for Research and Advanced Studies" (NIRAS), modeled after the present National Institutes of Health within the Department of HEW. If this recommendation had been implemented, the new agency would have administered all federal support for activities in the indicated categories, as compared with about 19% currently administered by NSF.[6] On the other hand, it would have fallen far short of being a department of science and technology, for even the present NSF is less exclusively academically oriented than the proposed NIRAS.

While the U.S. has been evolving towards an autonomous science agency, Britain and Europe have been moving in the opposite direction, attempting to bring their science activities into closer relationship to other policies and objectives of government. In Britain the conservative government moved to take several Research Councils out of DES and place them directly under the appropriate cabinet ministries. When this proposal met with considerable resistance, the government asked Lord Rothschild to prepare a report on government science organization. Its central recommendation was a compromise.[7] The Research Councils were to remain within DES, but they were to derive an increasing fraction of their budgets from the relevant ministries. Thus the ministries would act as customers, purchasing research services from the Research Councils according to their own specifications and estimates of their needs for new knowledge. The Research Councils would no longer determine what the ministries ought to need. If the ministries did not like what they were getting, they would be free to withdraw support, and even to create their own separate activities.

The Rothschild recommendations departed sharply from the so-called "Haldane Doctrine," dating from World War I, which stated as a cardinal principle that all government scientific activities should be conducted in

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autonomous organizations administered by scientists rather than by civil service generalists. Despite this philosophical change, however, the recommendations would reduce the percentage of non-military R. and D. supported by DES only from 28.4% to 20.0% over a period of several years, leaving a percentage still four times that supported by NSF in the U.S.[8] The purpose of the Rothschild recommendations was to insure that the work of the Research Councils, particularly their more applied activities, would be more responsive to the operational needs of the ministries, on the theory that he who paid the piper could call the tune.

It is interesting to observe that the present role of the British Research Councils within DES is somewhat analogous to the role which the RANN programs of NSF play in relation to the mission-oriented agencies. The advantage of such an autonomous agency is that it gives considerable policy initiative to a science-oriented agency. Such an agency is more likely to conceive and work on radically new approaches to problems than an operating agency bound by past experience and dependent on an external constituency for continuing political support in the annual budgetary process. On the other hand, its very freedom makes it vulnerable to the danger of re-inventing the wheel or expending effort on approaches to problems which more experienced and knowledgeable hands know to be impractical. The NSF-RANN approach tries to avoid these dangers by closely coordinating general policy through interagency committees set up within the Office of Science and Technology directly under the President's Science Adviser. These committees include top level representatives from the mission-oriented agencies most likely to be concerned and knowledgeable in particular problem areas to which RANN is addressing itself, such as energy and environmental protection.

On balance I feel that both RANN and the Rothschild recommendations are constructive, at least potentially so. No organizational changes can alone guarantee the solution of the interface problem between science and its applications, for this depends more upon individual personalities and motivations than on any kind of organization. On the other hand, organization can help to modify reward systems so as to change the nature of achievements. The RANN program does appear to fill an important gap between basic research and the civilian-oriented mission agencies which have limited experience in the sophisticated use of science to solve their problems. In the long run, however, RANN will succeed only if the contacts with the scientific community and the knowledgeable appreciation of scientific research characteristic of NSF in its administration of basic research can be infused into the mission-oriented agencies responsible for implementing results. Similarly, the recommendations of the Rothschild report will succeed only if the operational agencies develop their own scientific capabilities to the point where they are discriminating appraisers of the research they are obtaining from the Research Councils.

It may well be that, as the OECD report suggested, the best situation is one of dynamic tension between research in application-oriented agencies and research in science-oriented agencies. To quote the OECD report:

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Not only do we consider an either-or choice undesirable; we also feel that some degree of shift, with time, towards each of the two models may have a positive value in keeping the overall scientific system both healthy in its own right and more adaptable.[9]

Fragmentation vs. Dispersion of Research Effort

The argument over centralization vs. pluralism in overall government organization is paralleled by an argument over concentration vs. dispersion in research efforts at the working level. In some basic research fields such as space science, elementary particle physics, astronomy, and nuclear structure physics the increasing complexity and cost of instrumentation has forced scientists to concentrate in groups around the facilities, but even in these fields the U.S. has tended to favor the "user mode" of operation with a minimal staff attached to the facility itself and generous provisions for visiting scientists from universities and other research institutions.

The user mode is well illustrated by the situation in particle physics in the U.S. Here the operation and staffing costs of eight major facilities, plus accelerator development, consumed 71% of federal funds going into this field in 1971, yet 76% of the Ph.D. personnel doing particle physics research were located in universities, and 77% of the publications in the field originated in universities.[10] Forecasts of future development in other fields such as nuclear structure physics indicate a similar concentration of funding in a few locations even when expansion in a field is assumed to be limited by manpower rather than money.[11]

For problem-solving research there are equal or greater pressures for concentration because of the advantages of bringing many different skills and disciplines into close communication. Counter to this, of course, run the strong egalitarian pressures of American politics, best illustrated by the existence of a federally supported agricultural experiment station in each of the fifty states.

The AEC national laboratories are currently making a very strong bid for a major part in newly developing environmental and other societal R. and D. programs in the U.S.[12] Although a major motivation for this is simple bureaucratic self-preservation, their managers argue that they are in a better position than the universities to mount a tightly managed and at the same time diversified effort, particularly when it comes to coupling research to its potential uses. The universities argue, of course, that they are strong in the social sciences, while the large national laboratories are quite weak and would take a long time to develop comparable capabilities in these areas. Most of the AEC laboratories were created and organized by physical scientists, and it may prove difficult for them to create a climate sufficiently hospitable to social scientists to attract the highest quality people.

The large laboratories run into the dangers inherent in intellectual monopoly. Their excessively close communication and management tends to exclude the most radical and innovative approaches as threatening either because they are incompatible with official management doctrine and with the present hier-

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archical structure of the laboratory, or because they jeopardize its all-important relationship with its major sponsor. On the whole, I tend to favor a mixed strategy with a few major efforts as well as a definite and consciously protected margin of funds for the individual and less centralized projects which, as in basic research, have so frequently produced the most important new insights. Also in favor of considerable decentralization in research on societal problems is the fact that many of its applications have to be made in the context of a rather localized and specialized environment—in a particular city, or watershed, or local climatic stiuation. Thus regional research institutions are needed to adapt the generic findings of centralized institutions to the peculiarities of a local situation, and in return, their experience in doing so can give important guidance to the more generic research. This regional pattern appears to have been very successful in agricultural research, since it accommodated itself to the size of the U.S. and to its diversity of climate, soil, and hydrological conditions.[13]

Comparison of Government and Corporate Research Policy

Debate over the organization of science and technology at the national level is mirrored in discussion of the organization of R. and D. within large private corporations. A large corporation frequently creates a corporate laboratory as part of its central staff, supported by an assessment against the revenues of its various product or functional divisions. This laboratory is supposed to undertake research projects which are of general value to several product lines or functions, or which are so preliminary or so risky technically that an individual product division is unlikely to initiate them. Particularly important are projects likely to lead to new products or services which do not fit into any existing product division of the company. Even when an organization has a corporate laboratory, the product or functional divisions have laboratories of their own which undertake applied research clearly relevant to their own operational problems. These divisional laboratories help to make the divisions more sophisticated customers for the output of the corporate laboratory, but they can also be serious rivals to the corporate laboratory since, in the last analysis, they are competing for the same limited total corporate resources. The place of the NSF-RANN program in the federal government science structure is somewhat analogous to the place of the corporate laboratory in the structure of a large company, and it has some of the same problems and strengths.

Other large corporations do not have corporate laboratories as such, but have several strong divisional laboratories associated with various product lines, each reporting to the chief managing officer for the corresponding division. In such a corporation, however, the work of the strong divisional laboratories is coordinated by a vice president for research who reports directly to the top management of the company. These laboratories undertake on their own initiative some projects primarily related to the operational functions of

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the parent division, but they may also be assigned other projects aimed at developing new business or meeting a company-wide knowledge need, with the assignment governed primarily by the unique scientific capabilities of a particular laboratory. Thus, although they have no corporate laboratory as such, they may have, in addition to what amounts to a divisional research budget, a corporate research budget under the control of the vice president for research. The corporate research function is thus preserved as an overlay of the divisional structure. This model comes closest to that of the U.S. federal government, with the Office of Science and Technology in the Executive Office of the President playing the role of the vice president for research and his staff.

Probably the most fully developed example of the corporate laboratory model is that of the American Telephone and Telegraph Company, a private regulated monopoly. Here all research and development is centralized under the single management of the Bell Telephone Laboratories, supported half by the individual operating companies in proportion to their revenues.[14]

However, the Bell System found it essential to have, in addition to its central laboratory located near corporate headquarters in Murray Hill, New Jersey, a number of separate decentralized laboratories adjacent to various manufacturing operations and primarily supporting these operations, even though they remain under Bell Telephone Laboratories administrative and budgetary control. The Bell System management believes that this system of administrative centralization combined with geographical decentralization achieves the best blend of a coherent technical management and a responsiveness to the operational needs of particular parts of the company. It provides for a single scientific and engineering career system within the corporation, allowing technical people with new skills developed in the central laboratory to move easily into the operating arms of the company. The Bell System represents the best example of a highly integrated technical structure in a high-technology industry and is widely regarded as the most successful and innovative technical organization in the world. It is often suggested as an appropriate model for what a federal scientific organization might become.

However, there are pitfalls in trying to carry too far the analogy between the governmental and corporate situations. In the first place almost all corporate research is carried out "in house"; corporations seldom give contracts or grants to independent outside organizations the way the government does. Of some $17 billion the U.S. spends on federal R. and D., only 14% goes to civil service laboratories manned by full-time government employees. Another 5% goes to research centers, either profit-making or non-profit, whose facilities are wholly government-owned, but which are operated by a contracting entity which hires its own employees to carry out the work and is frequently managed by a board of directors or trustees created solely for the purpose. Such "Federal Contract Research Centers," or "captive laboratories," as they are called, are a hybrid of government and private organization.

Even counting the captive laboratories as "in-house," however, the federal

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government contracts most of its research out to organizations with different purposes and goals from those of its own agencies. Contracts or grants represent alliances for limited overlapping objectives. Thus they are not as responsive or as subject to close policy direction and to administrative and budgetary discipline as are the employees of a large corporation or its corporate laboratory. This is especially true in the case of universities and independent research institutes which together receive more than 15% of federal R. and D. expenditures. The program officers or contract monitors within government constitute a layer of technical management not present in the corporate analogy but required to couple the results of contract research to their final utilization in the decision-making or operations of an agency. The technical managers, in fact, identify in varying degrees with the goals of the contractor or the employing agency.

Extramural research has the advantage of access to a much wider variety of talents and ideas than the government could justify recruiting for its own purposes on a full-time basis. A corporate laboratory relies on the connections between its professionals and the general scientific community to provide a window on the whole potentially relevant world of science and technology outside the company. Contracted research provides a surer connection, but at a price of somewhat greater difficulty in coordination and coupling to end uses within an agency.

When applied or problem-centered research is administered by an agency such as NSF, the agency is really operating in the role of a corporate laboratory for the entire federal government. The kinds of research supported by NSF should be sufficiently generic and broad in scope to affect the mission needs of more than one federal agency or segment of industry. But support via NSF adds still another layer of management to complicate the coupling problem. Not only must NSF orchestrate, not too heavy-handedly, the efforts of many geographically dispersed and often fiercely independent grantees, keeping them aware of user needs, but it must channel all of their findings to potential users, and coordinate or reconcile the requirements of several different potential users. Much the same problem is faced by the Research Councils in the traditional British system of research management.

One of the questions that arises in this situation is how far NSF or Research Council research should be carried to insure a reasonable probability of its being picked up successfully by potential users and carried forward to final application. Unless there is considerable transfer of personnel, as occurs in the Bell System for example, the best research results may be lost due to the natural resistance of bureaucracies to new ideas, or to the "not invented here" syndrome. At the very least, considerable overlap in technical sophistication and perception of a problem is required between the producer and the user of new knowledge. The mere production of research documentation—published papers, reports, or lectures at professional meetings—is insufficient to insure its utilization even when it is demonstrably useful.

The second point on which the analogy between governmental and corporate

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research organization breaks down involves politics. All large organizations have bureaucratic politics, and governments and corporations are not too different in this respect. But in addition, government must cope with the politics of public opinion which have little parallel in the corporate world even in these days of large unions, active consumer groups, and public interest stockholder blocs. In the U.S. at least, the processes of government are increasingly conducted in a goldfish bowl. Recent legislation has opened up essentially all federal technical advisory activities to public and press observation, except when security classification or confidential personal information are involved. There is a serious question in my mind as to whether the scientific integrity of advice can be maintained under conditions where even tentative and subsequently discarded views may be subject to public scrutiny and the selective reporting of the media. The objectives of the new policy are, of course, to make decisions about science and technology responsive to the values and preferences of the public and to uncover and expose to public criticism the implicit value assumptions which often underlie ostensibly objective technical judgments. The difficulty with such procedures, however, is that they invite intervention by individuals and groups with very narrow interests, who bear no responsibility for the overall integrity of the resulting decisions or for the internal consistency of diverse governmental actions. The public exposure of the advisory process could easily result in a technical forum becoming a focus for contending political interests, and conducting discussions with little relevance to the technical issues involved. Recent congressional hearings on issues with a strong technical component suggest the difficulty of obtaining scientifically sensible results from such a process. However, in fairness, it must be added that the confidential processes by which advice is given to the executive branch have not invariably produced a much better result.

These two unique features of governmental research—the use of external contracts and the potential politicization of the technical decision process—make it difficult to draw unambiguous lessons for government from the experience of successful industrial research. Nevertheless, I believe that government can learn more than it has from the corporate experience. Regardless of the conflict between centralization and pluralism, there should be greater mobility of technical people between agencies in government. It should be easier for large laboratories belonging to one agency to undertake research on behalf of another agency when their particular technical skills warrant it. Government research planning should allow for more technical activity in the area intermediate between basic research and final end-item or operational development, and for parallel exploratory investigations prior to the choice of a preferred technical path.

New Challenges to Science and Technology

Today all the advanced industrial nations are suffering from disillusionment with the failure of science in attacking what are perceived as the most urgent

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problems of the future. It is increasingly appreciated that the blame lies not so much with science itself as with a lack of mechanisms and institutions to couple science to its operational applications in the social sphere.

For example, less than 2% of global R. and D. expenditures can be said to support economic development of the less developed countries. If poverty is regarded as one of the major global problems, then a pitifully small proportion of the world's technical effort is aimed at its alleviation. On the other hand, with present institutional and political structures, even if a greater amount were spent on R. and D. relevant to economic development, the results might not find their way into use. The bottleneck may lie, not in the lack of requisite knowledge, but rather in the lack of mechanisms to utilize such knowledge and diffuse it widely among the people and organizations that could benefit from it. In fact, the present level of such research may already be in equilibrium with what present institutional arrangements are capable of putting to use.

The failure in most spheres of economic development is underlined by the spectacular success only recently achieved in agriculture in Asia. In the first half of the 1960's it was widely believed that the world's poor countries faced imminent famine. The crisis was emphasized by extensive crop failures in India, and by an alarming increase in the less developed countries' need for food aid from the developed countries. Yet within five years India and the Philippines had become self-sufficient in their production of grains, and an increase in food supplies was comfortably outpacing population growth in many parts of the underdeveloped world.[15] This so-called "green revolution" was the direct result of adaptive research and the diffusion of technology initiated at relatively modest cost by private foundations in the U.S. The mechanics of this agricultural revolution include most of the factors commonly thought to be necessary for successful technological innovation: a basically sound technical idea based on an adequate background of basic scientific knowledge; good adaptive applied research in which potential users played an important part; receptive host governments and users, an organization for the diffusion of information closely coupled to the research organizations and designed to put knowledge into practice through the movement to the field of people closely involved in its original generation; and careful observation and feedback from the field of the results in operation.

However, this success did not spring full-blown from somebody's brain. It was based on long and successful experience in parallel circumstances: on the agricultural research and extension program in the U.S., and on the twenty-five year old Rockefeller program in Mexico. It was a multinational effort which had continuity and stability, and was relatively free from political or "participatory" pressures.

Once such successes are achieved in a limited field, the probability that they can be imitated in other areas increases. Success can create a demand for more R. and D., in a way that researchers themselves cannot. Unfortunately, few governmental programs outside the defense/space field have had the con-

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tinuity, stability of funding, political isolation, and opportunity for continuous organizational learning that seems to characterize the successful agricultural model and also the most successful innovations in the industrial field, including those of multinational corporations. The lack of political pressure for instant success has been one important factor in all of these successful innovations; at the same time, however, the pressure for long-term success created by the presence of eager and receptive users has also been important.

Conclusion

Everywhere in the developed world greater attention is being focused on the coupling of research to the needs of society, as opposed to the success of research on its own terms. The concern is especially with social problem areas in which past research efforts have been very modest and in which we have little experience to guide us as to the kinds of institutions and interface mechanisms which facilitate the successful application of new knowledge. In industrial manufacturing and in governmental military/space research successful mechanisms for the translation of research into application have evolved gradually during an extended period. The spectacular success of developed countries at translating research into applications in agriculture has recently been transferred to less developed countries through the green revolution. It remains to be seen whether it will take as long to evolve patterns which will enable us to fulfill needs which have only recently moved to the center of the world stage: to alleviate poverty and unemployment, to manage the world's resources and to protect its environment, to achieve balance between human populations and the integrity of the biosphere, and to deliver efficient and effective public services equitably to all people.

The central problem of scientific organization is how to reconcile the scientific need for autonomy and integrity in its own internal processes of exploration and self-criticism with the demands of society that the fruits of science be guided into channels which society deems beneficial. This is a problem to which there is no single specifiable solution. Rather the situation is one of dynamic tension between science and society, a tension based on an equilibrium configuration that will change with time, resulting, perhaps, in somewhat different configurations in different political systems. Government scientific organization has evolved differently in the United States from the way it has in Europe and the United Kingdom. There now appears, however, to be some convergence of the two patterns, with Europe moving towards a closer coupling of science to the agencies responsible for its application, and the U.S. moving towards isolating more scientific initiative in a single science-oriented agency, the National Science Foundation, which is capable of undertaking not only basic but also exploratory applied research relevant to a wide range of social problems. There are some dangers inherent in both of these changes. In the U.S. the greater engagement of scientific institutions, including the NSF, in problems with high political visibility could lead to the

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politicization of all of science. In Europe the introduction of science into agencies not ready to accommodate it may have similar effects. There seems little question, however, that the challenges facing mankind as a whole call for a high degree of sophistication and wisdom in the application of science, not only in generating new technology, but also in guiding the evolution of old fields of technology and in selecting new directions for technology by processes more judicious and less random than those of the past.

If there is any single theme of this paper it is that there are no either-or answers to any of the issues I have raised. I think we are dealing with dynamic tensions in which the victory of any extreme view is likely to be unhealthy, for part of the creativeness of the scientific establishment emerges from these tensions and from the challenges which science and society throw down to each other.

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LEWIS M. BRANSCOMB

Conducting and Using Research

How SUITABLE FOR future needs of world societies are the research institutions from which most technical knowledge flows? This brief paper cannot hope to answer such a broad question, but posing it will provide occasion for some observations on the nature of such research institutions and the circumstances that influence their effectiveness.

Even though it might be possible for scientists to assess the rate of progress of human knowledge under different institutional circumstances, societies do not—and probably should not—judge the social contribution of research institutions with reference to criteria and values entirely internal to science itself. Science is probably the most characteristic and unique expression of modern culture, but its magnitude and impact require a social justification which goes far beyond the intrinsic cultural value of the conceptual understandings it gives us.

Mankind enjoys ever increasing power to modify the world around him. When he intervenes in his environment for one purpose, his activities often produce unintended effects which grow much more rapidly than do the primary benefits which he intended. Such effects are called non-linear, and represent an increasingly visible by-product of man's technological prowess. In the past such effects have often caught us unaware, but today we have sufficient knowledge and understanding about our world so that we can no longer escape the responsibility for wise management of the future. More knowledge and understanding of the dynamics of both our natural and our social environments are necessary, but so are deeper insights into humanistic values and the human implications of our technological actions.

But knowledge alone will not be enough. Indeed, our most serious failure is not the inadequacy of our commitment to learning the truth; it is our failure to bring about action on the basis of that understanding to achieve the conditions for a reasonable future for the earth's inhabitants. The institutionalization of research reflects directly the mechanisms established by society for conducting as well as for using research. To assess their contributions to human welfare, we must examine their effectiveness in both these dimensions.

Thus, I will address myself to the question of how effectively research institutions create environments that facilitate the use of new knowledge, thus

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creating a valid demand—and hopefully the conditions—for further research. I will draw primarily on my experience in the United States, but I believe that many of my observations have a broader applicability. I begin by noting that the U.S. public is certainly ambivalent toward scholarly research as an effective means for solving practical problems. Some deride academic research as the solution of puzzles set by the researchers themselves rather than by the real world. Others reject the whole notion that problems can be approached rationally or analytically, preferring more intuitive or mystical approaches, usually because they are more holistic.

The time required for social and environmental change has become so short that the slow diffusion of ideas into general acceptance prior to their translation to social or technological action is a luxury we can no longer afford. When we came of age in scientific research, in the first half of this century, universities were the principal source of new technical knowledge. Traditionally, academic institutions have maintained a passive relationship to social problems, relying on the cumulative literature and the maturation of their students over a decade or so of time to work social change. It has become increasingly clear, however, that arrangements to create and use knowledge effectively are not so easily achieved as are the circumstances that ensure the perpetuation of scholarly journals.

It is now fashionable to focus on the application of research. There is in fact a genuine danger that the pendulum will swing too far towards applications, risking the intellectual viability of some of our research institutions before we have thought through how to improve their effectiveness. Of course the evolution of institutional patterns has in the past been very slow and we are still burdened by much conventional wisdom about the roles of university, industrial, government and independent laboratories.

Some say that certain types of research cannot be successfully performed in this or that type of institution, that, for example, fundamental advances in conceptual knowledge can only be made in universities; others say that universities cannot, or should not, engage in mission-oriented or problem-solving research. This is not necessarily true. Whether the organization sponsoring research is eleemosynary or profit-seeking, whether researchers also teach, or develop products, or administer public services is not of primary importance. What *is* important is: (*a*) Does the sponsor really need and want the research to be done? (*b*) Does the sponsor create the environment that fosters success in research and ensures the proper communication and use of results?

Many of the papers in this volume express a concern to retain the university as the principal home for basic research, but doubt that universities should engage in applied or mission-oriented research. While both concerns are justified, research is too important for human betterment to be left entirely to self-motivated researchers like many of those in the university. The largest part of research in science is undertaken for a purpose other than the addition to the sum of human knowledge or the training of future researchers.

If we judge research institutions by their capacity to foster the use of their

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output as well as to create new knowledge, we must be impressed by the stimulating power of insistent, immediate and continuing demand for research output from users close at hand. It is remarkable how slight a barrier to the coupling of research to applications can frustrate it. For this reason, most industrial laboratories subordinate their advanced technology work to organizational units which develop specific products. In industrial exploratory research laboratories, although they are somewhat insulated from day-to-day commercial pressures, management works untiringly to improve the likelihood that the research community will have the satisfaction of seeing its work brought to the market place. Companies that are successful at this coupling do not have to be persuaded to spend substantial fractions of their profits on research. Similarly, the most successful government laboratories closely integrate their in-house research efforts with their management of external development contracts.

In contrast, consider the sterility of most of the prestigious science research centers established by the U.S. aerospace industry in the late 1950's. Their glass and marble façades, now converted to more pedestrian uses, are monuments to a massive self-deception of which some scientists were the willing victims. This self-deception was the belief that in order for knowledge to be used or applied it merely had to be generated, that if the science were excellent enough, the world at large—and not just other scientists—would beat a path to the company's door.

One can find other examples of uncoupled—and therefore less than useful—applied research institutions. Many government laboratories have charters to do research on a topic of major social importance, but are denied the funds or authority to put this knowledgs to use for immediate public benefit. This situation is sometimes the product of a cynical political compromise between those who favor and those who oppose public sector intervention into new areas of need. A vigorous research program can be used to generate the appearance of forward progress, without the uncomfortable consequences, for adversely affected vested interests, of real progress. No wonder the public is frustrated by the evident impotence of research which is deprived of channels for beneficial application.

In the experience of the military as well as of the independent "think tanks" we find dramatic evidence of the effect of declining user pressure for high quality research to meet genuinely perceived needs. As the military establishment has turned inward to its "in house" laboratories to support its decision-making, the intellectual tone of the think tanks has sagged even more rapidly than their financial support. Indeed, I believe that real and sincere user demand is more critical to creativity in applied research than is the measure of the user's financial resources. The researcher quickly senses when what the user really wants is to create the appearance of intelligent decision-making, or to provide an intellectual rationale for decisions already arrived at on other grounds, and the integrity of his research suffers accordingly.

The applied researcher's satisfaction derives from *knowledge embodied in successful experience*. The experience of seeing their research used may be

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vicarious for most individual researchers, but it will often be real for the key individuals whose championship of an idea is the single most indispensable element in the innovative process. These individuals, called "champions,"[1] often stick with their ideas through the various levels of research. Champions, some of them independent inventors, have been found in government establishments, industrial research and product development laboratories, and universities. All of these research institutions have fostered successful technological developments when they were supported by impatient users with real and urgent problems and the minimum resources necessary to attack them. In addition to obvious examples from industry, we can cite the introduction of the precision optical glass industry to the U.S. by the National Bureau of Standards during World War I, the development of radar at M.I.T. during World War II, and the development of a great variety of agricultural products and techniques in U.S. land grant colleges. There is nothing intrinsic to the nature of a particular kind of research institution which precludes it from the successful accomplishment of any research task for which it is permitted to assemble the necessary array of talents. The experiences of World War II amply demonstrated that academic "pure" researchers, given the motivation and resources, could frequently outperform engineers trained more specifically in the supposedly relevant technologies. The guidance technology which made the Apollo moon landing and the Polaris submarine system possible was developed and carried to final application in what started as a purely academic laboratory created around a single M.I.T. professor. The first operational digital computers were designed and built in university laboratories. Conversely the first experimental demonstration of the wave nature of the electron and the first discovery of radio waves from outer space were achieved in an industrial laboratory. The emission of X-rays from certain kinds of stars was discovered in a government laboratory, as was the deciphering of the genetic code.

Hence, the important question is not which institutions are capable of undertaking various kinds of problems, but rather which institutions are most appropriate. To some extent, of course, this is a matter of their talent, facilities, motivation, interest, willingness to try something new, and freedom from other commitments. But primarily it is a matter of how demands by alternative users compete for the same intellectual resources.

What, then, is the role of the university in mission-oriented research? Some of the most urgent problems facing our society today require much stronger research inputs, much better connected to the decision-making process than those available in the university. Despite the National Science Foundation's program entitled "Research and Analysis for National Needs" (RANN),[2] it does not seem appropirate for universities to be asked to take responsibility for mission-oriented research. They can participate in basic research usefully, however, provided that some other institution, such as a Federal agency, provides the mechanism through which academic research can be put to use in solving a problem.

Fire research provides a good illustration. In per capita property loss and

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deaths from fires, the U.S. has the worst record by a large margin of all the industrialized nations for which data are available. The problem has been poorly analyzed; firefighting is done with primitive technology; political and institutional problems abound; and very little research, either physical or behavioral, has been done. This kind of problem could be illuminated by many kinds of academic talent, by economists, political scientists, physicists, engineers, architects, statisticians, chemists, and systems analysts. But the responsibility for the whole effort ought to be taken by a major laboratory capable of performing full-scale fire tests, of conducting and evaluating experimental or pilot approaches to organizing fire services, and of understanding the political leadership needed to obtain the cooperation of local operations in fifty states in providing basic data on the problem. This kind of program could well be managed by a government laboratory like the National Bureau of Standards (which has, in fact, the legal responsibility to institute such a program). If Congress provides the Department of Commerce with the funds it needs to direct such a program, university groups could participate usefully if faculties were willing to develop the necessary competence and experience. It remains to be seen, however, how strong university response will be.

Self-motivated university research alone cannot be relied upon to solve major social problems. Of course, with sufficient resources a university could over time develop or recruit the necessary range of talents and skills to do so, just as several universities did in the military or space fields during the fifties and early sixties, but it is questionable whether such an effort would be truly compatible with the other missions of the university—the development of new conceptual knowledge, and the education of students. Neither can the RANN program be relied upon, independent of other efforts, to solve our problems. But academic research could be coupled to the action agencies, taking as its particular province broad examinations of the way problems are conceived, and the identification of significant variables and major phenomena.

But the struggle to couple research to social benefits, which has correctly captured the attention of many university students, cannot be won on the campus alone. It must be fought in Congress, in state legislatures, and in Federal agencies to convince them to establish and fund effective action programs in government and private non-academic institutions. When political institutions come to understand the value of research the universities will be in a position to play an effective role. Regrettably, the research needs of health care delivery and environmental protection are only marginally met, and those in many other areas of great social importance, such as housing or urban transportation, are virtually ignored. Actually, the universities are doing much excellent scholarly work in these areas, but it tends to result in reports or lectures which go unread except by other university researchers or the intellectually alert segments of the general public, and has little influence on political or administrative decision makers.

The present dilemma of American universities is a reflection of their willingness, in the public interest, to attempt to fulfill all the demands placed

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upon them. Their past success in research—in basic and applied science and even in technology development—demonstrates that market forces alone must not be allowed to determine the university's function. For the primary client for the university's services, the student, does not exert any perceptible market demand for research activity explicitly appropriate to his needs.

After the student, the client most in need of the university's particular kind of independent competence is the polity. The most appropriate contribution of university research to social progress is to illuminate the decisions, public and private, that determine the course of human and, increasingly, of natural events. Universities can engage effectively in problem-solving but, with few exceptions, they have not combined problem-solving with their primary educational function successfully. The problem-solving unit tends in time to become an autonomous enclave within the university with a minimal connection to its educational or other research functions. But universities do have the talents, the freedom from commitment to particular technologies or social policies, and the intellectual courage to question whether discussions of public issues are addressing the right issues, giving adequate scope to imagination, and sufficiently examining implications for the more distant future. The most important asset the universities have to offer in the discussion and analysis of public policy issues is their long term perspective and their close connections with the frontiers of the basic disciplines.

Unfortunately we do not, in the U.S. (and I suspect the stiuation is even worse in other countries), have adequate financial support for this role. The resources available for university research are generated by a process that reflects conventional attitudes and a compromised point of view. Few corporate research sponsors are willing to pay to have their judgments sharply interrogated. Even fewer government bodies avail themselves of this most valuable service of research to human progress. A few years ago, for example, the University of Denver's Research Institute was prepared to undertake an analysis of economic and technological aspects of alternative policies for the development of oil shale resources. The University insisted, however, that neither the oil industry nor the Department of the Interior should provide a majority of the sponsorship. This self-imposed condition could not be met. Fortunately, such problems are normally somewhat mitigated by the pluralism of the government. When U.S. universities are sponsored by the government to engage in research to illuminate societal alternatives, that sponsorship tends to reflect not one but several competing interests because of the different agencies involved. The preservation of the intellectual freedom deriving from such bureaucratic competition is one of the powerful arguments against a monolothic department of science and technology.

My conviction that the public and its political leadership expect most research to be useful within a reasonable period of time has led me to the conclusion that research must be organized with full attention to the institutional mechanisms for translating research into social benefits. It does *not* lead me to the conclusion that all research must stand the test of utility. First, there is

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no doubt that a significant fraction of the U.S. population is genuinely fascinated by certain types of pure research, especially when the work is effectively communicated. This fascination is greatest either when empirical evidence raises basic philosophical issues as in cosmology or, at the opposite pole, when the scale or complexity of the technology itself is particularly striking, as in the space program.

Second, and more important, while applied research can and should be tested by its actual use (as well as by its utility), exploration for new knowledge is necessary, as a kind of societal infrastructure investment, before society's needs are identified. Furthermore, since exploratory research leads to undertakings in applied research, and since these in turn lead to technology, and since technology itself is a major stimulator of new discoveries, the whole procedure must be organized so as to foster this interplay while retaining the conditions for discovery and healthy progress in each separate type of research.

In order to organize in such a way, we must, in the future, experiment further with various kinds of cooperative ventures. Partnerships might be arranged, for example, between government laboratories and universities.[3] Joint technical studies could temporarily link an industrial laboratory with one of their customers or with a university. Within a single institution research scientists and development engineers could be usefully joined in technology benchmark projects that have clearly specified and measurable goals, and involve substantial technological risk, but do not aim at a specific market product. Such well defined benchmark projects might also be a good place for universities to become involved in the mission-oriented work of government agencies. As techniques for joining research to application improve—and much progress has already been made—the value of research will be increasingly well established. Institutional and national interdependence will increase, just as the global distribution of technology is accelerating.

To connect all of this activity, a network of user-oriented information systems must evolve, capable of linking the needs of the technical community to the intellectual resources for satisfying those needs. In spite of the facts that hundreds of millions of dollars have been spent on such systems and that their evolution has been analyzed in great detail by multinational[4] and international[5] bodies, the gap between current capabilities and future needs is enormous. Progress is impeded by the indifferent quality of information available in the literature, by the absence of adequate planning prior to initiating a research project for the use of its findings, by the conservative attitudes of scientists toward changing their communication habits, and by a lack of general appreciation for the monetary value of reliable information. In addition, the communication barriers between scientists and engineers are very substantial. For this reason their physical and organizational proximity is vitally important to the innovation process.

Fortunately, the technology exists to improve communication, and already new institutions such as information analysis centers are evolving that may eventually permit a much more efficient and less costly utilization of the world's

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knowledge. The rising fraction of national budgets going into research will eventually necessitate such efficiencies, as will the growing recognition that basic scientific knowledge must be internationally shared. Such an improved information system will have the consequences for research institutions of strengthening their interdependence and of confirming the value of a pluralistic institutional environment for research. Thus it will contribute not only to scientific progress, but also to international stability and harmony.

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2. RANN is an applied research grant program for universities. The title, "Research and Analysis for National Needs," might lead some skeptics to wonder if the remainder of the National Science Foundation grant program is for "research irrelevant to national needs." See papers in this volume by H. Brooks and D. Z. Robinson for further description of RANN.
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C. H. TOWNES

Differentiation and Competition Between University and Other Research Laboratories in the United States

A VARIETY IN institutions, with differences in style, facilities, personnel policies, motivation, and points of view is important to research because of the enormous variety in the paths to discovery. Discovery is, of course, inherently unpredictable. No one approach, regardless of how clearly it may illuminate some facets of knowledge, is likely to reveal all aspects even of a small field of science. Differences in style and purpose within and among university, industrial, government, or private research laboratories therefore represent more nearly an essential variety than several imperfect approximations to the perfect laboratory.

Yet these various research efforts overlap in important ways. They compete for outstanding personnel, for funds, and for research success in those areas where their efforts are applied to similar problems. Like variety, such competition is useful, as long as it does not wrest priority from efficient work towards the primary task of discovery. In this competition, there is a dynamic equilibrium among institutions which is reflected in the quality and number of their personnel, their available resources, and their research successes. Some aspects of this equilibrium have lasted a very long time. Others can easily shift within a period of a few years. Differentiation and competition are discussed here not in order to enhance competition, but in order to focus on a few strengths and weaknesses of different types of institutions with the hope of discovering useful modifications and adaptations.

Every nation has its own needs, style of work, and organizational techniques, and this differentiation has its function. The following discussion, however, is intended to apply specifically to the situation in the United States, and while it may have a good many parallels in other nations, certain details may be inapplicable elsewhere.

"Dynamic equilibrium" is necessarily an inexact description for what exists between laboratories and types of institutions. While personnel and funds can, in principle, shift from one type of laboratory to another within a year, in fact national policies and time scales are such that such changes normally take at least several years. Furthermore, most research is of such long-term nature that about five years is a minimum period for establishing a measure of accomplish-

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ment or failure. The driving force of national needs and policies, and the results in accomplished research, have a similar time scale for change, although they are often slightly out of phase.

Recently, in a trend which may continue for some time to come, the United States as a whole is facing a more rapid increase in the supply of research-oriented personnel than in the number of positions and apparent need for such people. It is also experiencing a much remarked change in the availability of financial support for research. These shifting conditions naturally cause readjustments in the optimum policies and relative successes of institutions. They also induce shifts of student enrollment in various research areas, shifts which may, by being out of phase with fluctuating demand, build the manpower disparities for the future.

Institutions engaged in research can be divided into four principal types: universities, industrial laboratories, government laboratories, and privately supported research institutions. In the category of government laboratory, I intend to include both laboratories run directly by a government agency and large centralized laboratories of unified purpose supported entirely by government funds, but run by some other agent. Under privately supported research institutions, I include those privately supported institutions which do research but essentially no classroom teaching. I would like first to compare three broad aspects of the various laboratories: their roles and goals, their facilities, and their personnel policies and attitudes.

The primary roles of the university are to teach and to develop new knowledge and insights. Provision of the highest levels of training in a given field almost necessarily leads to a concomitant development of new knowledge and insights. Students are exposed to the forefront of information and thought, and trained in developing new knowledge and insights themselves. Hence the teaching role of the university leads very naturally to a strong emphasis on basic research.

I will use the word "basic" to refer to work not directed towards an immediate practical application. The emphasis here must be on the word "immediate," since almost all basic work finds practical applications on some time scale and possible applications are always in the background. What has been called "strategic" research is basic work in an area judged to be useful for some specific purpose; hence it is directed towards practical applications, but not on any immediate or predictable time scale. The convenience and widespread use of the term "basic" does not mean that any sharp separation can be made between basic and applied research. Very frequently both motivation and results in a given project are mixed or intermediate in character, and often the support of research is motivated by interest in its applications but the work itself is carried out by people with attitudes which are basic in character.

Only private research institutions can have a commitment to basic research comparable to that of universities. Such institutions and their purposes are

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highly variable. Some of them, such as the Carnegie Institution of Washington or the former Rockefeller Institute of New York have been even more specifically directed towards basic research as a primary function than any university.

The role of industry in basic research is much more ambivalent. Its goal of producing a product or service effectively and cheaply naturally generates support for product development and applied research. The route from basic research to a company's product is usually long and chancy. Furthermore, even when new research knowledge is needed to develop a product, an industrial firm might obtain the information it needs most efficiently and cheaply from published literature or from others whose primary role is basic research.

In the long run, however, the value of research to industry or to the economy as a whole is often overwhelming, especially in industries where techniques and technology can change rapidly. Consider long-distance telephone communications. Between 1935 and 1965, the cost of a long-distance telephone call across the North American continent decreased by a factor of about four in actual dollars. In view of inflation, this was a reduction in real cost by a factor of about twelve, and since 1965 direct dial costs have decreased still further. While some of this cost reduction surely resulted from increased use of the telephone, much of it derived from research and development, some of it quite basic. A very large fraction of the applicable research and development took place in the Bell Telephone Laboratories, the dominant communications laboratory in the United States, a fact which enables one to specify its character and cost with some surety. The yearly bill in 1965 for all long-distance telephone calls in the United States was about $4 billion. With the knowledge and technology available in the mid-30's, the same service would probably have cost 25 billion dollars or more. For such service, present technology thus saves the American public many billions of dollars each year. The regulation of rates for the telephone system has also meant that steady reduction in cost of service provided one of the few routes for the company to increase its profits. How much did this research and development cost? The entire telephone system support of the Bell Telephone Laboratories from its beginning in the 1920's until 1965 was somewhat less than $2 billion. Thus, the decrease in cost of services each year is much larger than the total monetary support of the Laboratories over their entire history, and these contributions to long-distance telephony are, of course, only a part of the Laboratories' work. Among others, the chemical industry can produce parallel examples of the value of research and development.

In spite of such overwhelming figures in certain specific cases, for an individual company the decision whether and how much to support long-range research remains difficult. Because of the rapid diffusion of knowledge and the relatively small cost of patent rights, knowledge and technology can often be adequately borrowed or otherwise obtained more cheaply than by direct support of research. Even on a national scale, how much large-scale investment to make in basic research is not always clear, since one nation can profit from discoveries in another without having to make heavy investments in research

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itself. However, clearly any very large nation or corporation which hopes to be on the forefront of technology and effectiveness should make substantial investments in long-range research; a leading organization can hardly borrow most of its knowledge from those which do not lead.

In the industrial context, appropriate basic research can be expected to be of a largely "strategic" nature. Even about supporting basic research of a strategic nature, however, corporations often manifest ambivalence and vacillation. Basic research may be initiated in an over-enthusiastic hope of near-term applications, or in the belief that research will improve the corporation's image, or to induce highly trained personnel to associate with the corporation. Such conditional support is likely to be undermined whenever an industry is under economic pressure or undergoes a change of management. Unrealistic orientation of research programs coupled with short-term support has produced many disappointments both for management and for research personnel in industrial organizations.

The role and aims of government laboratories are rather more like those of industrial laboratories than of the universities. However, profits are invisible, competition much less in evidence than in industry, and stability usually greater. Consequently, longer-term and higher-risk research, of both basic and applied nature, are frequently supported in government laboratories. Unfortunately, the paucity of competition and the lack of profit accounting can also encourage tolerance of mediocrity and unproductiveness.

Assuming adequate funds are available, the privately supported research institution is free to adopt whatever goals and character its supporters and personnel may wish. When well financed and intelligently manned, it can have the great advantage of not being accountable to anyone else. Capable and lively personnel in private research institutions often create the conditions required for very outstanding success in basic research, but sometimes they suffer from lack of contact with problems of the world and with the flux of ideas.

As I have noted, the goals and roles of industrial and governmental laboratories are frequently congenial to applied research and to development. In these areas universities and many privately endowed laboratories are at a distinct disadvantage, because they are not so faced with specific applied problems to be solved, and do not have access to the large-scale facilities that are often important to applied research.

Research facilities of institutions may be described in terms of equipment, working space, and technicians or assistants. One overall measure of the cost of facilities and abundance of assistance is the overhead rate on salaries of research personnel, since it is this overhead which pays for most of their cost. By this measure, universities and industrial laboratories are in sharp contrast. The characteristic overhead rate in universities is about 50% of salaries, whereas that in industry is 100% to 200%. In this respect government laboratories are more comparable with industry, and private research institutions,

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while quite variable, are often more comparable with universities. Thus university research laboratories tend to be skimpy on technicians, services, and general facilities. Industrial laboratories are often outstandingly equipped as far as general facilities and certain kinds of assistance are concerned, although high-quality technicians are generally in scarce supply and hence not numerous even in many well-financed industrial laboratories.

Government laboratories also tend to have large amounts of equipment and a good number of supporting personnel. Often they have impressive investments in large-scale costly apparatus and equipment. Some of this may be unique, well planned, and well used, but in too many cases a piece of expensive equipment is poorly planned and manned, and comes uncomfortably close to being a white elephant. In certain cases, the primary function of a government laboratory is to build and operate some unique set of equipment to be available to the scientific and technical community at large. In other cases, an expensive or unique piece of equipment may be built apparently with the primary motive of upgrading the standing of the laboratory and attracting high-quality personnel. This phenomenon also occurs at times in industrial laboratories.

The lack of technicians and supporting personnel in universities is compensated at least partially by the supply of students who, while they may not initially be very expert, are likely to be bright and ambitious and to find apparatus construction and other chores involved in research very useful as a learning mechanism. In many fields, however, recent trends have decreased the number of research students, and may eventually result in inadequate manning of university research.

In the area of personnel policies and attitudes there are marked differences among the various categories of laboratories. These differences affect the attitudes and careers of individuals who populate laboratories, and also strongly influence them in their selection of a given setting for their work.

The dominant aspect of life in a university is commitment to teaching, which for research implies the flow of students and other young scholars in and out of the university laboratory. Both classroom and informal personal teaching distract the academic staff of the university from its research function. On the other hand, they provide a continual challenge of ideas and a demand for breadth which result in a research atmosphere of unique quality. The earlier stages of the careers of academic faculty are marked by a competition for recognition and favorable positions which may be as intense as the rather more publicized competition for executive positions in the industrial world. Later, the academic researcher tends to continue this habit of competition but without such intensity or any immediate visible effect on his career. The primary goals in this competition are research accomplishment, freedom and facilities for research and only occasionally administrative position. While the choice of research activities is strongly affected by the availability of facilities and of funds, there is seldom any university or departmental policy which

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directs the research of an individual faculty member. He is usually happily, but sometimes starkly, on his own.

In an industrial organization an individual scientist may, in some cases, have substantial freedom to direct his research, but at the very least he is expected to see that it is attuned to the needs of the corporation; in other cases, he finds himself closely directed by management to undertake a specific investigation, the nature of which may be changed on short notice. Much of the power and effectiveness of the industrial laboratory, in applied research at least, is connected with just this ability to direct a team of scientists and engineers and to enlist their attention and cooperation for intense work on a specific problem. It is a directed teamwork which is possible in universities only under very unusual circumstances, such as a war-time emergency. This very process, however, can be quite disruptive of sustained basic research and scholarly effort, and industrial organizations have considerable difficulty in maintaining both the strength of directed teamwork and the depth of scholarly approach required for outstanding basic research. In the industrial environment, frequently the primary competition is for administrative position, since it involves the authority to direct the efforts of a large team, or perhaps of many teams. Such laboratories have severe difficulty finding ways to properly recognize and reward a research person who may be outstanding in his field and creative, but a lone wolf with no wish nor suitability to move upwards in organizational and adminstrative responsibility.

An industrial laboratory which recognizes that it has a continuing need for knowledge and expertise in a particular field may be willing to support someone to devote a substantial part of his life to this area. His long-term commitment to this field can be valuable in that he has time to develop deep and detailed knowledge. Furthermore, he has a natural and frequent stimulus from applied problems. On the other hand, without teaching and the challenges of students and a variety of colleagues, or under proprietary pressures from his laboratory to maintain a certain distance from competitors, such an individual can become quite specialized and inflexible, missing opportunities to synthesize knowledge of his own field with that of others.

Personnel policies and attitudes in government laboratories vary widely, but are frequently intermediate between those of industry and those of the university. The competitive pressures typical of industry are not so omnipresent, so that long-term programs with rather stable goals and directions are common. Except concerning work to which access is restricted for military reasons, government laboratories are characteristically quite open and untroubled by proprietary restrictions on their communication with others. They may even be forced into frequent contact with other scientific groups if their function involves service to industry or to universities. While civil service policies favor administrative roles for able individuals, the difference in salary and standing between the successful individual research person and the manager is not as great in government laboratories as in industry. Frequently a government agency is quite protective of its laboratories and gives employees there advantages

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in the availability of equipment and services, which can be quite attractive to them. Thus, a stable government agency which establishes enlightened policies may provide a good climate for long-range and intensive research and thus produce very successful, steady, and systematic programs. On the other hand, the lack of intense competition characteristic of the early stages of academic life or of industry can also result in slow progress and mediocrity.

Private research institutions are so individual in their characteristics that their personnel policies and attitudes are difficult to describe with any generality. However, at least one characteristic is common to many of them. The most outstanding success of a private institution often occurs within a generation after it is first organized. If the institution is well endowed and its policies intelligently planned, it may be successful in obtaining initially very talented research personnel, both leaders in their prime and promising young people. These individuals are likely to make the institution particularly effective during their own active careers. However, once the institution has hired to the saturation point, it may gradually lose contact with active young people and find itself with an aging and somewhat stagnant research corps. Thus, without a substantial connection to university life or to active industry, and unless some major reorganization and rejuvenation occurs, such laboratories may find their most successful period lasts only about one generation.

My comments and generalizations are inadequate, of course, to represent all laboratories, some of which may have characteristics determined primarily by the genius of one or a few individuals, or by some particular setting. A part of a given laboratory may also blossom extraordinarily or be completely atypical because of individual talent or special historical developments. Yet I hope I have properly reflected a few important and common properties of research institutions. It is important to recognize that some of these properties, good or bad, are inherent in the type of laboratory and in its social and organizational context. However, efforts to capitalize on their better features and to ameliorate their inadequacies might well improve the productivity of each type of laboratory. Consider now some of the steps which may be indicated.

A measure which immediately comes to mind is that each type of laboratory might try to adopt the good features of others. One of the most successful ways of accomplishing this is by association; the carefully planned association of different styles of laboratories should be viewed as healthy symbiosis rather than as an unnatural marriage. For example, a university associated with a government or industrial laboratory may acquire thereby the stimulation of constant contact with applied problems, and also the large-scale facilities often necessary for applied research. Correspondingly the government or industrial laboratory acquires increased contact with talented young persons and the openness of a university environment. In some situations, such associations work well. In others, unsatisfactory arrangements or inequities in the strength of the two associated institutions may enable the stronger institution to dominate and injure the weaker one or may obviate any substantial in-

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fluence on and benefit to the stronger institution. The point of an association is to make available some of the helpful natural attributes of each institution without destroying the character of either. Privately supported research laboratories are flexible enough in character and goals to find association with universities particularly easy, they help to provide the university with facilities and outstanding personnel while benefitting from the breadth of interests and the flow of young people characteristic of the university. Very successful associations of government laboratories with universities are represented by the Lincoln Laboratories of M.I.T. and the Joint Institute of Laboratory Astrophysics at Boulder. Satisfactory close association between an industrial laboratory and a university laboratory is much more difficult to achieve, since it tends either to decrease the openness of the university or to amount essentially to a subsidy of the university by the industrial unit, a subsidy which an individual corporation cannot long support.

The need of university laboratories, particularly those doing applied work, for contact with real problems and with large on-going operations, can be partially solved by some other types of very loose association. One is the consulting for industry or government often done by university personnel. In applied work, or to maintain general breadth of experience within the university, this is a valuable and valid association. It should be cultivated, but with some care to see that it is of real value to the intellectual growth of the university staff and not so engrossing that it detracts from their devotion to educational work. Another partial solution which might be considered a distant association is the planned temporary exchange of personnel, and even the competitive hiring of research personnel away from one laboratory into another, with the resulting transfer of attitudes and experience. With the supply of trained personnel increasing and job openings decreasing, such exchanges may unfortunately be fewer in the future than during the last several decades. Research administrators would do well to find ways of facilitating useful exchanges of personnel for terms as long as a number of months, or a year.

University scientists had strong contacts with applied problems and with industrial laboratories during World War II when universities devoted much of their effort to military developments. This interaction between different types of laboratories and research personnel interrupted basic research, but it has also had important beneficial effects on research and research personnel in all U.S. laboratories involved in the physical sciences. The useful effects of these contacts are now fast disappearing and it is hoped that the type of military crisis which forced them will not be repeated. Perhaps another substantial contact of university personnel with applied work is developing at present as the result of interest in environmental problems. This is an area which will need for a while the open-ended and free-wheeling university approach as well as systematic long-range and detailed work which government laboratories can provide, and the directed, practical drive of industry. If national interest in environmental problems turns out to be long-term and substantial, it could be important both

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as a stimulus for new research and fresh institutional connections, and as an orientation for education.

A second general method for adapting the better features of one type of institution to the needs of another is direct adoption. Industry has, in some cases, simply tried to set up the motivational content of a university within its own house, giving relatively free rein to scientists for basic research and choice of problems. In cases where this has been tried in rather pure form, it has usually failed because the efforts and loyalties of research personnel became quite unrelated to the industry, and could not be permanently supported. In less extreme forms, it is successful in many cases—so successful that universities have sometimes found industrial laboratories difficult to compete with for the most outstanding talent.

It may seem peculiar that an increase in the effectiveness of one type of laboratory can be harmful to others, but competition makes this true. Hence at times competition needs to be thoughtfully compensated for on a national basis, or for that matter on an international basis when the research environment in one nation advances too far ahead of that in others. Such a situation arose, for example, following discoveries in solid state electronics during the 1950's. Outstanding basic research in solid state physics came to be so highly valued in industry that the most productive scientists trained in solid state work were provided not only with excellent facilities and relatively high salaries in many industrial companies but also with the quality of research freedom generally associated with universities, without the distractions of teaching or administration. The resulting competition for qualified personnel was so difficult for the universities that it posed a threat to continued top quality solid state research in academic institutions and hence to education in this field. The threat was ameliorated and reasonable competitive balance restored by the founding in key universities of special government-sponsored materials laboratories with needed facilities and support. In other branches of electronics, industry had from the beginning taken such a commanding lead in research facilities and opportunities that few universities could compete for the most outstanding personnel.

Privately supported research institutions have in many cases very successfully rejuvenated or kept fresh their research life by association with a university, by direct adoption of the practice of teaching within their own institutions, or by some combination of the two. Interesting examples are the Carnegie Institute of Washington, the Woods Hole Oceanographic Institution and the Rockefeller Institute, now Rockefeller University.

Universities have made substantial efforts to adopt at least one favorable feature of government and industrial laboratories by improving facilities. This has its value, but it has also encouraged a tendency of university research to move towards the use of large centralized equipment and shops. While this move has elements of efficiency and may be appropriate for other types of laboratory, it can weaken the university's teaching function by depriving students of firsthand experience with all elements of the research process. It

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is important for universities to resist overcentralization or overdependence on the centralized operation of large-scale equipment. Some types of research, of course, demand large-scale facilities. Too often, however, in work on large centralized equipment, the student is either engaged only in a minor facet of the entire enterprise; or he has the apparent privilege but the educational disadvantage of a user who requests that certain experiments be done and is then presented with the results, a process which allows him only second-hand contacts with important aspects of apparatus design, debugging, and operations.

University research could be facilitated by additional support personnel. Furthermore, shop and technical personnel closely associated with smallish research groups and available to interact intimately with students can be very effective in familiarizing students with basic techniques.

At present, university departments are typically populated by a combination of students, young Ph.D.'s, and faculty members primarily oriented towards their own personal research. Too few supporting and intermediate research personnel stay permanently in the university, although they could be invaluable in maintaining continuity of programs and skills and in giving assistance to students in technical or specialized aspects of research. If the number of advanced science students continues to decrease, such personnel would be especially important in maintaining successful ongoing research programs, directed by the teaching staff. Such supporting technical personnel could serve effectively on a part-time basis. Hence positions of this type would be appropriate for women with young families, providing them with continued contact with front-rank research.

Industry needs first of all to find ways of establishing clearly and consistently just where its interests in research lie. Hopefully, once it does so, the larger corporations at least will give stable support to strategic as well as to applied research. Most of the smaller corporations should probably not maintain substantial research efforts at all. They do, however, need clever work on product development, which might sometimes be labeled research. Such work can in fact be the lifeblood of a small technologically-oriented company. However, uncertainties and fluctuations in the rewards of longer-range investigation may be too great to justify its consistent support within a small company; furthermore, the disadvantages both of small size and these uncertainties tend to undermine the effectiveness of whatever modest long-range research is undertaken. Larger corporations can accept risks and fluctuations in the research game, and indeed those which use technology extensively must maintain successful and alert research laboratories if they are not to become antiquated and non-competitive. Can this give them too dominant an advantage over smaller corporations? Yes, in many cases it could in the absence of public concern and of adequate sharing of technology.

Industrial laboratories must seek personnel policies which recognize the value of individual senior scientists who do not undertake management as a career,

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and which allow them to participate actively in the scientific community. In universities, a department chairman frequently works energetically to see that salaries of outstanding professors within his department are increased far beyond what he expects to receive himself. By contrast, the traditions and relatively structured life of an industrial laboratory make it psychologically and practically difficult for an administrative boss to see that an outstanding research person under his direction gets a salary consistently greater than his own. But such a phenomenon is not impossible, and might provide some needed recognition for creative research personnel. Recognition by his peers is probably still more important than a large salary so that giving him opportunities to mix easily with the national and international community of scientists and to publish freely may be the best way to recognize him, and at the same time, to provide the contacts and criticism needed to keep the industrial laboratory alert. Exchange of personnel with other types of institutions, particularly universities, can also provide some of these opportunities and breadth to industrial laboratories.

On the other hand, we should not expect that the industrial scientist will characteristically be a generalist in the same sense as a university scientist and teacher tends to be. Instead, if he is consistently supported by industry, the industrial scientist can have an unusual opportunity to cultivate deeply a particular field and to become outstandingly expert. While the management of the industrial laboratory must maintain its effectiveness in coordinating intensive efforts of teams of research personnel, particularly in the area of applied research, it must also find ways to maintain research of a different quality, of the longer-range and more individualistic variety which requires at least partial insulation from the immediacy of daily demands and shifting crises of industrial life.

Although industrial research laboratories differ widely in quality and in their attractiveness to outstanding research personnel, for the most part they have had difficulty competing with the more prominent university and private research institutions for high-quality research personnel oriented towards basic work. The present slow-down in university hiring, mostly the result of a normal growth and maturation cycle, has created a job shortage which, though difficult for such personnel, may benefit other kinds of research institutions by providing a better balance in availability of highly qualified personnel, thereby creating an important opportunity for other laboratories with suitable openings and research policies.

Government laboratories serve a number of unique functions. In some cases, laboratories are created to make needed attacks on problems whose national importance has rather suddenly become evident. A centralized government laboratory can be built quickly to focus research on such problems. In other cases, laboratories must be maintained in areas where an agency has an important interest and needs its own experts both to carry out some of the research involved, and properly to review, oversee, or even direct efforts of

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other laboratories. In still other cases, central facilities are needed either for a research area of importance to a government agency or for the general use of the scientific community. However, the large resources sometimes available to government laboratories and the frequent lack of real competition in research performance are special hazards for them.

Industry normally competes with universities for personnel by offering high salaries, good facilities, and possibly administrative power; government laboratories, however, generally offer competition to universities only on the last two scores. Agencies of the government frequently gravitate in the direction of maintaining unnecessarily expensive and not particularly productive equipment. In many cases, the difference is enormous between the supporting facilities available to an individual in a government laboratory and those normally available to him in a first-class university; this disparity exists even in laboratories operated for the government by universities. Perhaps some such differences are essential if the government laboratories are to continue to exist and carry out their functions. However, in many cases such differences are so large that they are probably not tolerable over long periods of time.

A government agency is naturally concerned about the fate of one of its own laboratories, and may hence be overindulgent. This instinct is particularly hazardous when the same agency, perhaps even the same individual is responsible for initiating or maintaining the laboratory, as well as for making decisions on competing bids by other institutions for research support. There are of course valid reasons for some laboratories to have much better facilities than others and different styles of operation. However, some of this unhealthy disparity often evident between government laboratories and others could probably be avoided by administrative care to remove the decisions on granting agency funds for supporting research from those who have a special and personal interest in the maintenance of one of the agency's own research laboratories.

Like a private research institution, a government laboratory may be very effective while young, but become stagnant sometime later, particularly if the problems for which it was initiated have become less urgent than when they first attracted attention. Despite political and human difficulties, support should be removed from those laboratories whose mission has been completed or has disappeared and which cannot be effectively reoriented to work on current vital problems. One of the more successful government laboratories, the Naval Research Laboratory, has been in operation a long time. In addition to the continuing viability of its mission, its success seems closely related to maintenance of high quality basic research and close contact and competition with the rest of the scientific community. Methods must be found to enhance such contacts, particularly for government research centers not already closely associated with universities, and to challenge continually the efficiency and success of government laboratories.

The decreasing availability of university positions and a growing tradition in government laboratories of employing young postdoctoral personnel for a short period after they receive the Ph.D. degree seem to offer real promise

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for maintaining a flow of younger people through such laboratories, and for giving them experience with the values and opportunities there.

Clearly, government laboratories should cultivate their unique roles and advantages. They can serve well, I believe, in any long-term systematic work of either specialized or broad interest to science which is not carried out by industry. Such long-term systematic work is increasingly difficult to maintain in universities. A steadily growing need of science and technology is for the critical collection and codification of information in the many fields which have been advancing so rapidly during the period of vigorous growth and enthusiasm for science over the last several decades. While university personnel may be the most obvious source of textbooks, monographs and critical tables fit naturally into the roles of many government laboratories. The effort needed to digest information, carry out systematic series of measurements, and make the results better available for use is large, but it could be of enormous value to further progress everywhere.

There should be special concern in government circles and in the technical community for the health and efficiency of the laboratories of the Department of Defense. Some of these are indeed on the forefront of research and some are working away very effectively on longstanding problems, more or less unnoticed by much of the technical community. But too many have long suffered from stagnation and mediocrity, a fact which, coupled with some isolation due to military classification and the disinterest of much of the scientific community in perfectly real and valid military problems, may in coming years make these laboratories particularly ineffective and wasteful. Both the administration of the Department of Defense and the technical community have a responsibility to find ways of eliminating some and refreshing other such laboratories.

The ingredients of a successful research environment, and the difficulties of research administration are clearly even more complex than my already involved discussion indicates. Two more factors must at least be mentioned: the crucial role an individual can play, and the powerful effect of tradition. These factors, though not simple to analyze, are easily illustrated—the first strikingly so by the fructifying effect of Enrico Fermi on Italian physics and physicists, or of Ernest Lawrence on the University of California. The second factor, tradition, creates an expectation and ambiance which are often valuable but slow to change either for better or for worse. To illustrate this I shall be quite personal. One of the clinching arguments for me when I accepted my first job, at the Bell Telephone Laboratories, was that Davisson and Germer had done their famous experiment there. After I transferred, a decade later, to Columbia University where I. I. Rabi's influence was then enormous, I found still a few bright-eyed young men who had been stimulated to come to Columbia by the interesting figure of Michael Pupin, dead more than a decade and long before that finished with any active role in science.

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PETER KAPITZA

Basic Factors in the Organization of Science and How They Are Handled in the U.S.S.R.

THIS, IT IS SAID, is the age of the scientific and technological revolution. Science, this implies, with its effect on technological progress, is the principal motive force in contemporary culture and civilization. That is the reason why the development of science is a major concern of nations today, especially of advanced nations. It has become clear that for scientific research to develop effectively in a country, it must have a special very specific organization, different from all other organizations. Questions related to the organization of scientific research are now attracting the widest interest among scientists themselves, for only scientists are capable of fully understanding and evaluating the specific features of organization necessary for the efficient development of scientific work in their fields. Experience has shown that the more involved a country's scientists are in the organization of science, the more successfully science develops in that country.

Organizational measures for the development of scientific research fall roughly into three main groups.

1. Selection and training of scientific personnel.
2. Choice of directions of scientific research appropriate to requirements of world science and to the cultural and industrial requirements of a given country.
3. Provision of the necessary material resources for scientific research, such as research centers, an instrument-manufacturing industry, and information exchange.

These measures, of course, are handled differently for different fields of science. Then, too, the economic and social structure of a country affects the organization of its science.

However, science is indivisible and in the final analysis its achievements are the property of all mankind. The international character of science encourages cooperation among scientists of all countries and establishes a world standard for the evaluation of scientific research. Thus, it is hardly surprising that one factor, perhaps the most significant of them all, is equally important to the healthy development of science in all countries.

This factor relates to the first and most important of the above-mentioned

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measures for the organization of science: the selection and training of scientific personnel. Experience has shown that, even though it is not yet getting the attention it deserves, it is this factor more than any other that ensures a high level of effectiveness in scientific research.

Science is a field of human activity which must be pursued by creatively talented people if it is to advance. It is common knowledge that only a small number of people have enough creative talent to make a mark in art, literature or music. The same is true of science. Hence, for science to develop successfully, it must, like art, have a supply of gifted people. However, as one can easily see, this supply is harder to come by in science than it is in art, where life itself does the selecting and where assessment is not connected with special organizational difficulties, for a poor novelists's novels will not be read and no one will listen to an untalented composer's music. It is much more difficult to evaluate a person's creative achievements in science, though evaluation here too is made by a kind of public opinion, that of a small group of specialists. Therefore, selection of creative scientific personnel requires the establishment of a special organization which can utilize the evaluation of specialists in the field. This is one of the most difficult organizational problems of scientific research.

The number of scientifically creative people may differ in different countries, but it is always a small proportion of a country's total population. On the other hand, the scientific and technological revolution requires the broadest possible development of scientific research, and this calls for a large number of scientific personnel. It follows that the main objective of the organization of scientific work is to make sure that a country's available talent is used correctly. The first step, then, is to create a situation that will attract gifted people to careers in science. The second is to devise a system for selecting interested people in accordance with their talent. And the third is to create conditions for training talented young people that will enable them to develop their potentialities fully.

Today all advanced countries have specialized secondary and higher schools in art and music—academies of arts, conservatories of music—to which students are admitted at as early an age as possible, as soon, in fact, as they begin to show talent, which talent is thereby given the opportunity for the fullest development. As yet, no such opportunity is available to scientifically gifted young people, though the need for it exists in many countries.

Because selection and training of creative scientific personnel is the basic factor determining the effectiveness of the organization of scientific work in a country, I shall devote most of this article to it and describe what is being done about it in the U.S.S.R.

The question of the selection and training of young scientists has comparatively little to do with the social structure of a country, for in both capitalist and socialist countries education of the young is in the hands of the state. Therefore, the questions under consideration here are of general significance.

Except for slight variations, the system of selecting and training scientists used most widely in the world today is similar to the one used in the Soviet

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Union. Future scientists attend universities and higher technological schools together with other students, and only afterwards and on the basis of abilities they have shown there, are they admitted to graduate schools to do scientific work. There, for the first time, they engage in scientific research, and only then do they have the opportunity to show their creative abilities. The writing and defense of their theses for the Kandidat degree (a Soviet intermediate degree, roughly equivalent to a U.S. doctorate) will be used to judge their suitability for creative scientific work.

This system has several major shortcomings. The principal one is that a person is already between twenty-three and twenty-five years old, a mature adult, before his gifts begin to be discovered and developed, despite the fact that a creative talent for science, though it surfaces at a later age than artistic talent, is usually clearly discernible by the age of eighteen or nineteen, that is, by the time a person completes secondary school. (Pure mathematics is an exception; there, as in music, creative talent may manifest itself at a much younger age). Consequently, these creative abilities have no opportunity to develop during the four to five years of higher school, precisely those years when the brain is most receptive.

Another shortcoming of this system is that very few higher educational establishments have research scientists on their faculties, and experience has shown that ordinary teachers do not have the qualities needed to train and assess scientific talent. The present system of training scientists may have been acceptable in former times when only a small number of scientists was required and little significance was attached to the effectiveness of scientific research. As a rule, the most talented young people were drawn to science, but had to push their way through to it on their own, irrespective of the routine schooling they received. However, with the increasing scope and demand for scientific research today, it is high time that specialized higher schools were set up to educate and train scientists, like those set up long since for musicians and artists.

In Moscow, the first higher school of this kind was established twenty-five years ago—the Moscow Physics and Technological Institute. It trains scientists in various fields of physics and has a present enrollment of 3,000. It was founded on the initiative of a group of eminent, active physicists, chemists, mathematicians, and other scientists associated with the most intensively developing fields of science and technology and responsible for the scientific and technological revolution.

I participated very closely in the organization of that institute and should like to describe, in some detail, the principles on which we founded it, the problems which confronted us, and how we handled them.

The first, and probably the most difficult problem was the selection of students for admission to the institute. In approaching this selection problem, the important thing was to create a system of entrance examinations which would disclose a person's creative talents.

Secondary school aims to provide its students with a certain quantity of knowledge according to a specific program; success there is determined by how

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much of this knowledge the student learns. Secondary school does not develop or assess a student's creative abilities. Therefore, successful completion of secondary school is not a criterion good for selecting young people for scientific careers.

The system of selection we adopted is based on competitive exams, as it is everywhere, but the exams have certain features that enable the examinee to display his creative abilities. Much significance is attached to the choice of examiners. The majority of them are young and all of them are gifted scientists, for we believe that only active scientists are capable of correctly evaluating creative talent. In grading the examinations in basic subjects, we pay special attention to how the student has handled the problems in mathematics and physics. Then comes the crucial test for the applicant who has received at least passing marks in the exams: an interview with a group of two or three examiners. The purpose of the interview is to discover the applicant's level of sophistication, the nature of his talent, and whether he has aptitude for scientific thinking. The interview is held informally so that the examinee may not be prevented by shyness or nervousness from expressing his individual qualities.

The competition for admission to our institute is intense: the 500 available places are sought by eight times that many applicants. Such intense competition gives us a great deal of material from which to select. The quality of our applicants is also affected by the fact that creatively talented young people usually have a natural desire to make a career in science and therefore seek to get into our institute.

Selection of future scientists does not end with admission to our institute, however. Another selection takes place after students complete a two-year study program, consisting mainly of such fundamental disciplines as mathematics, experimental and theoretical physics, mechanics, stresses of materials, and languages. There is a great deal of emphasis on seminars and lab work. We try to obtain professors and teachers whose primary occupation is doing research in research centers, to lecture and to conduct seminars in their disciplines, and also to determine the general organization of instruction. Here again we proceed from the assumption that only scientists are capable of creatively instructing and training young people in science.

After he completes the two-year program, a student's selection depends on a state examination in physics, designed not so much to determine how much he has learned, as to find out how well he can use the knowledge he has acquired. The examination is conducted in an unusual manner. It is an oral exam. The student is given his questions and problems ahead of time and is permitted to consult books, magazines, notes, even faculty members, before the examination. What is required of him is the kind of understanding of the field of physics that will enable him to use his knowledge in dealing with the problems he is given. Moreover, the problems themselves are posed in unusually general terms. The student is required to indicate the path a solution might follow and to give a quantitative evaluation of this solution. He is given a choice of questions to help the examiners determine both his capacity for in-

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dependent interest in scientific problems and the depth of his comprehension of them. An examination of this complexity is an improvement in itself over previous methods of selection of young people for scientific careers.

An average of about ten per cent of the students fail at this stage and are given the opportunity to transfer without examinations to the third year of any ordinary college or university of their choice.

The students who pass this examination now continue their training outside the walls of the educational institution. They are sent in small groups to specialized research laboratories of their own choice, usually located at one of several dozen large scientific research centers which we call *bazismy*, or foundational institutes. A chair is created at each of these centers for a supervisor to oversee the further training of students in accordance with a specific program and also to organize the students' participation in research. The only classes students now attend at the academic institution are those in general subjects such as economics and languages. All training in special disciplines takes place at the foundational institute.

There the students are in the hands of young and, for the most part, gifted scientists who lecture and conduct seminars. Usually only five to fifteen students work in a single area of research, first as laboratory assistants, later in more independent work. The student takes his exams and writes his lab reports at the research center during his training period of three years, after which he writes and defends his graduation thesis.

The approach to each student is individual. The full course usually takes six years, but if a student is unusually capable he may, with the approval of the research center executive body, complete and defend his thesis sooner. There have been students who have completed their work a year, or even a year and a half, ahead of schedule.

On graduation, the student is officially designated an engineer-physicist and is usually offered a research position in a scientific institute. By the time a student finishes the course I have described, he has not only acquired scientific research skills, but has also been able to discover the bent of his talents. Consequently, he should be able to decide what kind of work he wishes to do after graduation.

The most gifted students may enter a still higher program for improving their skills in scientific research, known as internship. The foundational institute usually selects a few of its most capable students to keep on as probationers after they have defended their graduation theses. After two more years at the institute, if a probationer shows himself to be an able researcher, he may be offered a permanent position on the staff or he may go on to graduate studies. The graduate school course is three years, after which the student defends a thesis and earns the degree of Kandidat of Sciences. Thus, the selection and creative training of scientists are constantly combined with their actual involvement in research, and our experience has shown that this is the best way to train scientists who will go on to do good work in the future.

In the twenty years since our Moscow Physics and Technological Institute

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was established, our close follow-up on the work of its alumni has confirmed us in our belief that the above system for training talented young people is much superior to the system in operation in other technological institutes and in the universities.

It should be mentioned that our system can still move in a number of directions. Many young people, for example, have talents which extend in several directions, some of which are not being developed at present.

Furthermore, although we generally recognize two basic directions in physics, and accordingly train both theoretical and experimental physicists, the way physics is developing, this division is no longer sufficient. The present huge scale of experimentation requires the construction of such highly complex apparatus as enormous accelerators, chambers for observing the flow of elementary particles, nuclear reactors, complex electronic devices for space research and superconductor solenoids with creostats. To produce these it is necessary to be an engineer as well as a physicist. The need has therefore arisen to train physicist-designers.

In the very near future, we also have to deal with the problem of training scientist-organizers to head research groups. Many extremely important problems in contemporary science can be solved only through research by large groups of scientists. To succeed, these require coordination by a talented organizer—one who is not only a fine scientist, but a fine leader as well, gifted with the kinds of talent required of a manager in industry. The creative talents needed by the head of a scientific collective resemble those of the director in a contemporary film. The director must select and harmonize the performances of a large cast of actors; his organizational talent ensures the success of the film. Nowadays the talent of the director is recognized at least as much as is that of the leading actors. Analogously, there are large scientific research groups, in space research for example, in which the leading scientist-organizer plays a major role, one on which the success of the entire group depends. Yet, as a rule, the success of a scientific achievement is unfairly attributed exclusively to individual scientists. Surely this will have to change in the future, for the leader and organizer deserves high appreciation. Furthermore, such chiefs will have to be selected from among scientists and given special training in scientific management. And this must become a major objective of the organization of science.

The continuing scientific and technological revolution is constantly increasing the scope of scientific research. Statistical data indicate that both the U.S.S.R. and the U.S.A. have close to one million persons working in organizations connected with scientific research, and that this number is increasing. Since the effectiveness of scientific research is determined primarily by the creative abilities of the scientists engaged in it, and since the number of young people with a talent for scientific research is limited, one of the most important organizational tasks of science is to attract more and more gifted young people to careers in science. To do this, we must make the situation of the scientist sufficiently attractive. Not only must he be well provided with material things, but, even

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more important, he must be assured a high and respected position in society. To one degree or another, of course, his situation is attractive in all advanced countries today.

Much can still be done to improve selection of scientifically talented young people. The examination-interviews of our institute do not, for example, guarantee that the largest possible number of the country's most creative young people will find their way to scientific careers. The selection process could be improved if it were transferred to the last few years of secondary school.

The difficulty here is that the professional teaching staffs of ordinary schools are not adapted to educate their students creatively. Hence, the only way to exert a creative influence on secondary school students is to organize it from the outside. One of the best ways this is done at present is through the organization of what we call *olympiads* for high school students. These are contests in mathematics and physics, instrument design, astronomical observation, and other areas of interest in science and technology, and they are designed to reach as many students as possible. Organized by groups of scientists, they not only spotlight talented young people, but also encourage children to take an interest in scientific creativity from an early age. Contests of this sort are widespread in our country, as they are in the U.S., and this is a good thing.

Another way of encouraging interest in science is through the extracurricular circles in various subjects which universities and other higher educational establishments offer secondary school students. Led by young scientists, these circles familiarize the high school student with the creative scientific process. However, they reach relatively few secondary school students because they are feasible only in cities which have colleges and universities.

Finally, there is a plan afoot to get young scientists, mainly graduate students, to teach part time in the upper grades of secondary schools. In fact, the idea is to make it obligatory for graduate students to teach such subjects as physics, mathematics, biology and chemistry in secondary schools. Not only will this develop the native talents of the secondary school students, but it will help us to discover the best among them early. This would also benefit the graduate students, for it is common knowledge that teaching is the best test of one's own knowledge.

The second organizational problem confronting science is how to choose areas of research appropriate to the basic directions of world science and to the country's cultural needs. The body in charge of this choice must be able to judge the importance of scientific themes, to determine the quality of scientific researchers, and to evaluate scientific results already achieved.

Clearly only a group comprised of scientists is capable of performing this kind of organizational function; in fact, everywhere academies of sciences, scientific associations, academic councils, and similar representative groups are put in charge of this type of work. Everywhere, too, such organizations operate democratically. First they discuss the problems confronting them, then deter-

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mine their decision by a vote. This is the generally accepted system for handling problems connected with scientific work and is, apparently, the sole correct one. It is especially effective for evaluating scientific personnel. Consequently, all major positions in scientific institutions are filled by secret ballot. Only the placement of rank-and-file scientific personnel is done by one-man decisions at the administrative level. This system does, however, tend to be more centralized in the socialist countries than in capitalist countries. For example, in the U.S.S.R., the Academy of Sciences is a central organization with very broad powers. It appoints by secret ballot the directors and top level scientific personnel for all of the country's leading scientific institutions. Though it is the council of each scientific institution which confers the academic degrees of Kandidat and Doctor after passing on dissertations, a special Higher Certification Committee strictly supervises the procedure. This committee is comprised of scientists and it, too, makes its decisions by secret ballot.

In socialist states, the selection of scientific themes to be pursued is also to a certain extent controlled by a centralized body of the scientific community. The Academy of Sciences guides the work of all of its institutes (which are engaged primarily in research on problems of pure science) by means of its power to approve or veto all research plans. Problems connected with technology, agriculture, industrial output, and the like are handled in institutes under the various ministries and in the State Committee of Science and Technology. These institutes are guided by their own scientific councils which also operate on democratic principles.

Obviously, the selection of areas of research is more centralized in socialist states than in capitalist countries where it is determined by executive bodies in the interests of large industrial concerns. The centralization that characterizes the socialist system has a number of major advantages. It is, for example, easier to watch out on a nationwide scale for duplication of research, to make immediate use of scientific achievements on a nationwide basis, and to coordinate large numbers of scientific institutions run by different ministries to do research on pressing problems. But centralization also has its weak points. The slack time involved in implementing plans may become quite lengthy. In addition, centralization is susceptible to bureaucratic stagnation. Of course it is possible to combat these weak points, the most effective weapons against them being community supervision and criticism.

The significance of achievements in science has always and everywhere been assessed through the discussion of scientific papers by colloquies of scientists, not only at the level of the councils of scientific institutions, but also more broadly, at conferences, symposiums, and congresses. International meetings of scientists recognize contributions to world science and make highly objective evaluations which often exert considerable influence on directing bodies within nations.

The efficient exchange of scientific information by journals and correspondence and, most importantly, by personal contacts among scientists and visits to scientific laboratories, is one of the most effective ways of ensuring correct evalu-

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ation and direction in research. International cooperation among the scientists and scientific groups of all countries engaged in research not only helps to guide research effectively, but also ensures that science adheres to a world standard.

There are some who believe that the practical implementation resulting from the achievements of basic scientific research can be used as a criterion for evaluating the original basic research. In my view, this is so only in a limited number of instances. The fact of the matter is that it takes such a long time for industry to assimilate a scientific achievement, especially a major one, that research in progress is hardly affected, for the group of scientists involved changes direction before the assimilation is completed.

The third problem pertaining to the organization of science concerns the provision of material resources for scientific research. Money is a prime resource, money for paying scientists and auxiliary personnel, for constructing and equipping research centers, for organizing the exchange of information, and for all kinds of peripheral expenditures. In socialist countries the allocation of financial resources is planned centrally. In capitalist countries, some of the money comes from the government and some from private sources. In both cases a question arises as to how much money should be allocated for science as a whole.

It is known from published data that approximately three percent of gross national income is allocated for scientific work in both the U.S.S.R. and the U.S.A., as well as in a number of other advanced countries. We have no reliable way of determining whether this is indeed an optimal proportion, but it has now been established beyond any doubt that when allocations for science increase beyond it, the number of scientific personnel increases, but the effectiveness of money expended begins to decline. As a matter of fact, some say that the effectiveness of a research institute's work is in inverse proportion to the logarithm of the number of scientific personnel. They argue that science gets the most capable and talented researchers first, and that, inasmuch as the number of creatively gifted people is limited, quality must decline when too many researchers become involved. In my view, a country's success in science is dependent fundamentally on the number of creatively talented people it can tap.

For a scientist to perform efficiently, a certain average sum of money must be spent on him annually (for a physicist, this sum is about $30,000.) Therefore, since we know the number of persons with talent for scientific research and we know the sum that has to be spent on each of them, it is possible to determine the amount of money a country can productively spend on science. It may very well be that three percent is correct. However, should our efforts to improve the selection and education of talented young people succeed, eventually, in increasing the total number of competent scientific researchers, that sum would also have to be increased.

If the effectiveness of scientific research could be determined as definitively

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as that of industrial production, we could determine how much money should be invested in it merely by solving a problem in arithmetic. Calculations of this kind have, in fact, been attempted when research was closely linked to practical industrial problems, but they have never been very convincing.

The quantitative effectiveness of a scientific institution's work might, indeed, be determined as follows. Assuming that scientific research is creative exploration, and that unsuccessful attempts usually precede the solution of a problem, we could take a ratio of the number of successfully solved problems to the number of total attempts to solve them as the coefficient of the effectiveness of scientific research. Since this coefficient will inevitably be small for difficult major problems, it cannot be regarded as the sole indicator of the quantitative effectiveness of a given scientific group's work. However, when the coefficient remains at zero, the scientific institution concerned should be closed down.

I doubt that it is possible to devise a method for the quantitative evaluation of scientific creativity, any more than it is possible to do so for creative art. We have not been able to solve much simpler problems. We cannot quantitatively compare, for example, the achievements of major individual scientists whose work is well-known and universally recognized, say, of Newton and Einstein. It is as impossible to do this as it is quantitatively to compare the work of two great artists like Rembrandt and Picasso.

Nevertheless, present efforts to study the processes of scientific creation and to evaluate specific scientific achievements may be very useful with regard to the organization of science. It will no doubt emerge that the most important factor ensuring the effectiveness of scientific development in all fields of knowledge is the ability to attract the most gifted young people to science and correctly to select and train them.

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Notes on Contributors

J. B. Adams, born in 1920 in Kingston, England, is Director-General at CERN, Laboratory II, Switzerland. He has done extensive work on particle accelerators in nuclear fusion research.

Joseph Ben-David, born in 1920 in Győr, Hungary, is professor of sociology at the Hebrew University, Jerusalem, and visiting professor of sociology at the University of Chicago. He is the author of *The Scientist's Role in Society* (1971), and *American Higher Education: Directions Old and New* (1972).

Lewis M. Branscomb, born in 1926 in Ashville, North Carolina, is Vice-President and Chief Scientist of the IBM Corporation. He has served as Director of the National Bureau of Standards, and taught at Harvard University; University College, London; the University of Maryland; and the University of Colorado. He has published numerous papers on such diverse topics as upper atmospheric physics, spectroscopy, the photodetachment and physics of negative ions, and astrophysics.

Harvey Brooks, born in 1915 in Cleveland, is Dean of Engineering and Applied Physics and Gordon McKay Professor of Applied Physics at Harvard University. He is a member of the National Science Board and has served as a member of the President's Science Advisory Committee, and as Chairman of the NAS Committee on Science and Public Policy. He is the author of *The Government of Science* (1968), and of numerous articles and reviews on solid state physics and nuclear reactor physics.

Ronald Dworkin, born in 1931 in Worcester, Massachusetts, is professor of jurisprudence at Oxford University and a member of the faculty of the Yale Law School. He has worked in the law firm of Sullivan and Cromwell and served as master of Trumbell College at Yale.

Sir John C. Eccles, born in 1903 in Melbourne, Australia, is professor of physiology and biophysics at the State University of New York at Buffalo. Among the many honors he has received are Knight Bachelor, 1958; Nobel Prize, 1963; and Mitglied Deutsche Acad. Naturforschung and Cothenius Medal, 1963. His works include *Neurophysiological Basis of Mind* (1953), *Physiology of Nerve Cells* (1957), *Physiology of Synapses* (1964), *The Cerebellum as a Neuronal Machine* (1967), *The Inhibitory Pathways of the Central Nervous System* (1967), *Facing Reality: Philosophical Adventures by a Brain Scientist* (1970).

Northrop Frye, born in 1912 in Sherbrooke, Quebec, is University Professor in the University of Toronto. Among his numerous publications are *Fearful Symmetry* (1947), *Anatomy of Criticism* (1957), and *The Educated Imagination* (1963).

Sir Ernst H. Gombrich, born in 1909 in Vienna, is Director of the Warburg Institute and professor of the history of the classical tradition at the University of London. His works include *Caricature* (1940), *The Story of Art* (1950), *Art and Illusion* (1960), *Meditations on a Hobby Horse* (1963), and *Norm and Form* (1966).

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HARRY G. JOHNSON, born in 1923 in Toronto, is professor of economics at the University of Chicago and at the London School of Economics. Among his many publications are *Macroeconomics and Monetary Theory* (1972), and *Inflation and the Monetarist Controversy* (1972).

PETER KAPITZA, born in 1894 in Kronstadt, Russia, is Director of the Institute for Physical Problems of the Academy of Sciences in Moscow. He is the author of *Collected Papers* (1964-1967) and of various publications on physics, mainly on magnetism, cryogenics and high temperature plasma. In addition, he has written a number of articles on the organization of science. He is one of the founders of the Moscow Physico-Technical Institute, and the editor of the *Journal of Experimental and Theoretical Physics.*

S. E. LURIA, born in 1912 in Turin, Italy, is Institute Professor and Sedgwick Professor of Biology at the Massachusetts Institute of Technology. In 1969 he shared the Nobel Prize in Medicine. He is the author of *General Virology* (1953 and 1967), *Life: The Unfinished Experiment* (to appear in May, 1973), and of 150 other papers on microbiology and genetics.

DAVID Z. ROBINSON, born in 1927 in Montreal, is the Vice-President of the Carnegie Corporation of New York. Previously he served as Vice-President for Academic Affairs at New York University, as a member of the staff of the President's Science Adviser, and as Assistant Director of Research at Baird-Atomic, Inc.

C. H. TOWNES, born in 1915 in Greenville, South Carolina, is University Professor, affiliated with the department of physics, in the University of California. Among the numerous awards and honors he has received is the Nobel Prize (for his role in the invention of the maser and laser). Dr. Townes is Chairman of the Space Science Board of the National Academy of Sciences.

ERIC WEIL, born in 1904 in Parchim, Germany, is professor of philosophy and director of the Centre d'Histoire des Idées at the University of Nice. His works include *Logigue de la Philosophie* (1967), *Philosophie Morale* (1969), *Hegel et l'Etat* (1970), *Problèmes Kantiens* (1970), *Essais et Conférences I* (1970), *Essais et Conférences II* (1971), and *Philosophie Politique* (1971).

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